THE LONG-RUN EFFECTS OF CHILDHOOD INSURANCE COVERAGE: MEDICAID IMPLEMENTATION, ADULT HEALTH, AND LABOR MARKET OUTCOMES

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Abstract:

This paper exploits the original introduction of Medicaid (1966-1970) and the federal mandate that states cover all cash welfare recipients to estimate the effect of childhood Medicaid eligibility on adult health, labor supply, program participation, and income. Cohorts born closer to Medicaid implementation and in states with higher pre-existing welfare-based eligibility accumulated more Medicaid eligibility in childhood but did not differ on a range of other health, socioeconomic, and policy characteristics. Early childhood Medicaid eligibility reduces mortality and disability and, for whites, increases extensive margin labor supply, and reduces receipt of disability transfer programs and public health insurance up to 50 years later. Total income does not change because earnings replace disability benefits. The government earns a discounted annual return of between 2 and 7 percent on the original cost of childhood coverage for these cohorts, most of which comes from lower cash transfer payments.

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In 2014, the joint federal and state public health insurance programs, Medicaid and the State Children's Health Insurance Program, covered 40 percent of children and cost $475 billion. Costs have been central to recent arguments about the size of the Medicaid program (Sommers and Epstein 2013). Current federal budget proposals would convert Medicaid into a block grant program, several states have implemented major cuts to eligibility and services, and six states have recently considered opting out of the program (Adamy and King Jr. 2010).

Short-run empirical evaluations show that Medicaid improves health, but this has not resolved debates about the program's future. For example, while Medicaid saves lives (Currie and Gruber 1996a, b, Goodman-Bacon forthcoming, Sommers, Baicker, and Epstein 2012), the health effects are small for middle-income groups, and costs per life saved can be high. Experimental estimates from the Oregon Health Insurance Experiment show improvements in self-reported health measures but not in clinical measures, providing support for both Medicaid's advocates (Kishore 2014) and critics (Antos and Capretta 2014, Roy 2014). Therefore, short-run health effects may not justify the size of the program (Finkelstein, Hendren, and Luttmer 2015).

Accounting for Medicaid's effects over the course of its recipients' lives may drastically change this cost-benefit calculation. Because it primarily covers children during critical periods, Medicaid may have its largest effects later in life (Cunha, Heckman, and Schennach 2010). Improvements in adult health and economic outcomes could also *lower* public costs by reducing transfers or health care spending in programs linked to poor health, or by increasing tax revenue.

New research based on eligibility expansions from the 1980s shows that Medicaid can have positive long-run effects on health, human capital, earnings, and tax payments (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014, Miller and Wherry 2014, Wherry and Meyer 2013, Wherry et al. 2015). Yet these studies observe cohorts in their 20s, so longer-run effects, especially those tied to health conditions that emerge at older ages, may be significantly larger or smaller than existing estimates. Direct estimates of longer-run effects are central to conclusions about the return to Medicaid spending, though. When calculating Medicaid's return on investment, for example, Brown, Kowalski, and Lurie (2014) assume that effects at age 28 will persist for 32 years, and this assumption accounts for three quarters of the estimated return (42 of 56 cents per dollar, p. 21).

This paper provides new evidence on Medicaid's longer-run effects by exploiting the program's introduction between 1966 and 1970 and the federal mandate that Medicaid cover all cash welfare recipients ("categorical eligibility"). These two program features led to a sudden increase in public insurance eligibility that was larger in areas with higher welfare participation. From a long-run perspective, cohorts born closer to Medicaid spent more years potentially eligible for it, and those from higher-welfare states had a higher eligibility rate in each year. Thus, *cumulative* childhood Medicaid eligibility phased in gradually across cohorts but more quickly for those from higher-welfare states.

To estimate the effect of cumulative Medicaid eligibility on adult outcomes, I use a difference-in-differences model that compares cohorts born at different times relative to Medicaid implementation in states with different categorical eligibility rates in the year *of* implementation. This initial welfare rate provides a fixed way to compare states with different levels of pre-Medicaid categorical eligibility. Variation in initial welfare rates came from long-standing institutional features of states, and was uncorrelated with levels or trends in economic, demographic, health, and policy characteristics; nevertheless, this variation strongly predicts cumulative Medicaid eligibility and contemporaneous Medicaid participation (Goodman-Bacon forthcoming). Comparing adult outcomes across cohorts born in different years relative to Medicaid implementation and in states with different initial welfare rates is therefore unlikely to confound the program's effects with other health, socioeconomic, or policy changes.

Event-study specifications support the validity of the design by showing the relationship between initial eligibility and adult outcomes for *each* cohort born up to 30 years before and five years after Medicaid implementation. Outcome variables are measured by state and year of birth and include cumulative mortality rates from 1980-1999 (using Vital Statistics Mortality Files), self-reported disability rates, labor market status, transfer program participation, and the distribution of earnings and transfer income (from the 2000-2014 Census and American Community Survey). These outcomes track patterns of eligibility closely: they are uncorrelated with initial Medicaid eligibility for respondents who are too old to have qualified as children; diverge gradually in higher- versus lower-eligibility states for cohorts with increasing years of exposure; and flatten out for post-Medicaid cohorts with the same predicted childhood eligibility.

The results show that cohorts with early-life Medicaid eligibility experience lower adult mortality and disability rates. Among white adults, these health improvements reduce the use of disability benefits and public insurance. New earnings largely offset lower transfers, leaving individual income unchanged, a similar form of program substitution as that identified by Kline and Walters (2016). The government, on the other hand, saves on benefit payments *and* earns a small amount of new income tax revenue: $21.5 billion per year in total relative to a total cost of childhood coverage of about $132 billion in 2012 dollars. At a standard 3 percent discount rate, these changes imply about a 7 percent return *every year* on the initial investment. Using observed treasury rates to discount the costs and benefits implies about a 2 percent return and suggests that, between 2000 and 2014, the government recouped about 28 percent of the (true) original cost. Almost 60 percent of this return comes from reductions in cash transfer payments, and the remainder comes from increased income tax revenue (28 percent) and lower public insurance spending (13 percent).

I. Expected Effects of Medicaid Implementation on Later-Life Outcomes

Medicaid's original introduction provides an especially clean context in which to study the program's long-run effects. Before Medicaid, private insurance was rare among the poor, public medical programs were small, and free sources of medical care were uncommon and often of low quality (Goodman-Bacon 2015).[1] As a result, poor children frequently went without medical care. Figure 1 shows that fewer than half of poor children in the early 1960s had seen a doctor in the previous year relative to three quarters of middle-income children.

Poor children were also strikingly unhealthy in ways that extended into adulthood. Their mortality rates were twice as high as those of non-poor children (National Center for Health Statistics 1965), and they suffered more often from a range of specific symptoms.[2] In terms of adult health, one highly publicized 1964 report showed that over one-quarter of Army inductees were rejected on medical grounds, most commonly for diseases and defects of the "bones and organs of movement" (President's Task Force on Manpower Conservation 1964). The report's

[1] Only about eight percent of adults received any free care in 1960 (Morgan et al. 1962), and only 2.8 and 13.4 percent of low-income children in non-Medicaid states had doctor or clinic visits (respectively) without charges in 1969 (Loewenstein 1971, p. 2.11 table 2.31). 9 percent of respondents (with children) in the 1968 PSID reported that they could get "free care." Anecdotal evidence suggests that free care was low-quality and hard to obtain. A 1964 Children's Bureau report describes a hospital outpatient department in Dallas, Texas as "deplorable." In Birmingham, Alabama "many [are] turned away from outpatient clinic (40 or more a day) due to lack of funds…a mother returned with her dead baby in a sack" (Lesser et al. 1964). One hospital administrator in New York City bemoaned the passage of Medicaid, asking "How do you expect [continuing medical research] to be carried out if patients come to the hospital only for medical care and are not interested in taking part in new and as yet unaccepted methods of treatment?" (Stevens and Stevens 1974, pp. 99).

[2] Parental reports of specific disease incidence appear not to provide reliable measures of disease burden. In the 1963-1965 National Health Examination Survey (USDHHS/NCHS 1991), for example, higher-income children are more likely to report having mumps, bronchitis, scarlet fever, polio, allergies, or a heart murmur. However, poor children are more likely to have symptoms that are observable without a diagnosis such as a sore throat, colds, a "heart problem," or identifiable conditions such as whooping cough.

"most significant finding" was that these differences were correlated with socioeconomic status and that "*75 percent of all persons rejected for failure to meet the medical and physical standards would probably benefit from treatment*" (italics in original, pp 25).

A. Medicaid Implementation, Children's Insurance Coverage, and Aggregate Utilization

Medicaid's passage as title XIX of the 1965 Social Security Act Amendments represented a major expansion in the availability and generosity of (publicly funded) medical care for poor children relative to the small existing federal/state medical financing system for welfare recipients. Medicaid removed federal reimbursement caps, increased federal matching rates, defined a set of required medical services (inpatient, outpatient, physician, lab/x-ray, and nursing home) and mandated coverage for recipients of cash transfer programs (the "categorical eligibility" requirement). Almost all categorically eligible children (89 percent) qualified through the Aid to Families with Dependent Children (AFDC) program (DHEW 1976). All states except Alaska (1972) and Arizona (1982) implemented Medicaid between 1966 and 1970.

During Medicaid's first decade, states also expanded their efforts to identify and screen children for debilitating but treatable conditions. The Early and Periodic Screening, Diagnosis and Treatment (EPSDT) program required states to locate eligible children and "ascertain their physical or mental defects, and [provide] such health care, treatment, and other measures to correct or ameliorate defects and chronic conditions discovered thereby" (PL 90-248 quoted in Stevens and Stevens 1974).[3] President Johnson stressed EPSDT's potential later-life effects when he advocated for the program: "Ignorance, ill health, personality disorder—these are disabilities often contracted in childhood: afflictions which linger to cripple the man and damage the next generation" (Johnson 1967).

Immediately following Medicaid implementation, public insurance coverage among children increased sharply while uninsurance rates fell. Less than one percent of children had public coverage in 1963, but about 15 percent did by the mid-1970s, and almost all of this increase reflected reductions in uninsurance (Goodman-Bacon 2015, figure 1).

[3] Stevens and Stevens (1974) discuss lags in the promulgation of EPSDT regulations in the late 1960's but emphasize that the program was a major new proposal, requiring screened children to receive a "full health history, an analysis of physical growth, developmental assessment, unclothed physical inspection, ear, nose, mouth, and throat inspection, vision testing, hearing testing, anemia testing, sickle cell, TB, urine and lead-poisoning testing, as well as nutritional and immunization status reports" (pp. 257, note 50). They also cite an early experience in Mississippi in which "1300 abnormalities were discerned in the first 1200 children screened" (quoting Howard Newman, pp. 257 note 51).

The large increase in coverage meant that poor children received substantially more medical care. Appendix table 1.1 presents cross-sectional differences in utilization by Medicaid eligibility across 10 surveys from before and after Medicaid implementation showing that children eligible for Medicaid used much more medical care than ineligible poor children in the same state, or similar children in non-Medicaid states.[4] Figure 1 shows the net result of these utilization increases: the steep income gradient in children's doctor visits in the early 1960s almost completely disappeared by 1975.

B. State Differences in Contemporaneous and Cumulative AFDC-Based Medicaid Eligibility

Because Medicaid—through its categorical eligibility requirement—built on existing cash welfare systems, it inherited large, long-standing state-level differences in the size of these programs. Therefore, while all states experienced a sudden increase in public insurance eligibility when Medicaid began, this jump was much larger in states with higher welfare participation. Patterns of AFDC participation also differed strongly by race. For example, 1.3 percent of white children in Nevada were eligible through AFDC when the state's program began in July 1967, but 5 percent were eligible when New Mexico's program started in December 1966. For nonwhites, differences in initial eligibility are reversed: 22 percent in Nevada versus 10 percent in New Mexico. I therefore stratify the analysis by race to capture the distinct variation in white and nonwhite initial eligibility.

The sharp change in eligibility after Medicaid translates to a phasing in of cumulative eligibility across cohorts born in the years leading up to implementation. A white child born (and raised) in Nevada in 1950 had two childhood years under Medicaid and a 1.3 percent eligibility rate at age 17, and a 1.6 percent eligibility rate at age 18. Her expected number of years of *full* eligibility is 0.029.[5] A similar child from New Mexico, however, had higher eligibility and three years of exposure, for 0.17 years of cumulative eligibility. For the 1960 cohort, born 10 years closer to the start of Medicaid, cumulative childhood eligibility is 0.47 for Nevada and 0.85 for New Mexico.

[4] Appendices are available here: http://www-personal.umich.edu/~ajgb/medicaid_longrun_appendices_ajgb.pdf

[5] All eligibility measures refer to the expected number of *full* years of Medicaid eligibility. Treating 1967 as a full year of implementation and assuming that the monthly AFDC participation rate in Nevada of 1.3 percent is constant, then the expected number of *months* of eligibility in 1967 (the interval at which AFDC eligibility is actually determined) is 12*0.013 = 0.156, which is the same as 0.013 full years of eligibility. Because of churning in AFDC caseloads, the expected number of years with *any* Medicaid eligibility is higher.

To construct a cohort-level measure of cumulative childhood Medicaid eligibility, I use state-by-year-by-race data on AFDC rates, statutory Medicaid implementation dates, and migration information in the Census. The number of expected years of childhood Medicaid coverage for a cohort born in state *s* in year *c* (of race *r*) is a weighted sum across the years of childhood *and* the states of residence (ℓ) of that cohort:

$$m_{rsc} = \sum_{y=c}^{y=c+18} \sum_{\ell} \sigma_{rsc}^{y}(\ell) \cdot 1\{y \geq t_{\ell}^{*}\} \cdot AFDC_{ry\ell} \qquad (1)$$

$\sigma_{rsc}^{y}(\ell)$ is the share of cohort *c* (race *r*) born in state *s* living in state ℓ in year *y*.[6] The Medicaid implementation dummy, $1\{y \geq t_{\ell}^{*}\}$, equals one if year *y* is after state ℓ's Medicaid implementation date ($1966 \leq t_{\ell}^{*} \leq 1970$). $AFDC_{ry\ell}$ is the observed AFDC rate for children of race *r* in state ℓ in year *y*.[7] Since 89 percent of Medicaid children qualified through AFDC, $1\{y \geq t_{\ell}^{*}\} \cdot AFDC_{ry\ell}$ is a close proxy for annual child Medicaid eligibility.

Differences in contemporaneous AFDC rates led to wide variation in cumulative childhood Medicaid eligibility. Figure 2 plots m_{rsc} for cohorts defined by year of birth relative to Medicaid implementation in their *birth* state. After Medicaid, white children gained about a year of eligibility on average and nonwhite children gained about five years (solid line), but differences across states in cumulative Medicaid eligibility are about as large as the average increases. Cross-state variation in AFDC also strongly predicts Medicaid *participation*: a one percentage point difference in AFDC rates when Medicaid began led to a 1.9 point increase (s.e. = 0.4) in annual child Medicaid utilization for at least six years after (Goodman-Bacon forthcoming, appendix figure 2B.4).

C. Expected Longer-Run Effects

A large body of evidence suggests that this rapid growth in insurance coverage and medical care use should affect health and economic outcomes later in life. Infant and child health are strongly correlated with test scores, education, labor supply, earnings, and welfare receipt in adulthood (Currie, Decker, and Lin 2008, Smith 2009). Early life exposure to specific infectious diseases negatively affects adult health, education, and earnings (influenza: Almond 2006,

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[6] Using the state of residence and 5-year migration variables in the 1970-2000 Censuses, I can calculate $\sigma_{rsc}^{y}(\ell)$ every five years starting in 1965. I linearly interpolate between these observations to obtain birth-state-by-birth-year-by-calendar-year estimates of the state of residence distribution.

[7] Age-specific AFDC rates are not available at this time, but the 1970 Census shows that welfare participation rates are essentially constant during childhood. Details on the calculation of race-specific AFDC rates are in appendix 1.

malaria: Barreca 2010, pneumonia: Bhalotra and Venkataramani 2015, hookworm: Bleakley 2007, gastrointestinal disease: Chay, Guryan, and Mazumder 2009, 2014, meningitis: Roed et al. 2013, typhoid fever: Beach et al. 2014). A number of mechanisms could link child and adult health, including an inflammatory immune response (Crimmins and Finch 2006), a diversion of nutritional resources (Fogel 1997), reduced energy and school performance (Adhvaryu et al. 2015, Bleakley 2007), direct organ damage, or changes in parental investments (Becker and Tomes 1976). Evidence on Medicaid implementation, and the categorical eligibility requirement in particular, shows substantial short-run improvements in child health (Goodman-Bacon forthcoming). Medicaid reduced infant deaths through improved hospital care with no discernible effect on health at birth, and reduced deaths, mainly from treatable infectious diseases, among young children.[8]

Recent work uses both state-by-year variation and a birth date discontinuity in the 1980s eligibility expansions to estimate Medicaid's effects across the life course and finds striking improvements in health and economic outcomes. Childhood eligibility is associated with improvements in both teenage health (obesity, BMI: Cohodes et al. 2014, self-reported health: Currie, Decker, and Lin 2008, mortality: Wherry and Meyer 2013) and adult health (mortality: Brown, Kowalski, and Lurie 2014, obesity, BMI, chronic illness: Miller and Wherry 2014), and with reductions in adult hospitalizations for chronic disorders (Wherry and Meyer 2013, Wherry et al. 2015). Medicaid's long-run benefits extend beyond health to academic achievement (Levine and Schanzenbach 2009), educational attainment (Cohodes et al. 2014), and earnings (Brown, Kowalski, and Lurie 2014).

These results, however, may not provide a good guide to Medicaid's longer-run effects because the cohorts affected by the 1980s expansions are mainly observed during their 20s. The longer-run effects could grow if Medicaid reduces the lifetime incidence of chronic conditions, or could fade if Medicaid simply delays the age of onset.[9] The only direct evidence on effects at older ages is mixed. Using the differential timing of Medicaid adoption across states, Boudreaux, Golberstein, and McAlpine (2016) find that, among adults who were poor in 1968, childhood

[8] The lack of contemporaneous effects on health at birth rules out a fetal programming explanation for any long run effects. However, acute care at birth can, itself, improve later-life outcomes (Bharadwaj, Løken, and Neilson 2013).
[9] The age profile of chronic illness suggests that Medicaid's effects could change drastically after age 30. National Health Interview Survey (NHIS) data show that chronic conditions such as hypertension, diabetes, cancer, and arthritis strike adults aged 30-64 more than five times as often as adults aged 19-30, the typical age range used in existing long-run studies (Minnesota Population Center and State Health Access Data Assistance Center 2012).

Medicaid exposure leads to higher scores on an index of adult health outcomes but not on an index of economic outcomes.[10]

In summary, the contemporaneous effects of Medicaid's introduction appear to have triggered life-course health processes that matter for adult outcomes in other contexts, including more recent Medicaid expansions. Medicaid's origin thus provides an opportunity to understand the program's longer-run effects in a way that has not previously been possible.

II. DATA: MEASURING ADULT OUTCOMES BY STATE AND YEAR OF BIRTH

I first examine an extreme but objective health outcome: cumulative mortality rates. I construct 20-year mortality rates conditional on living to 1980, by dividing the count of deaths from 1980 to 1999 from the Multiple Cause of Death Files (United States Department of Health and Human Services 2009) for a given race-by-birth-state-by-birth-year cell by the corresponding population estimate from the 1980 Census. Summing deaths over 20 years increases power relative to shorter time periods. Furthermore, race reporting among Hispanic Census respondents changed as the number of race categories grew, but this did not always occur on death certificates, which are filled out by medical examiners or funeral directors. Matching "white" and "nonwhite" deaths to similarly coarse denominators in 1980 avoids time-varying misclassification (Arias et al. 2008). For cohorts born close to Medicaid implementation, these mortality rates cover a range of ages between adolescence and mid-40s.

The primary mortality outcome is the log of 20-year mortality rates from non-AIDS causes. During the 1980s and 1990s, more adults aged 25-49 died of AIDS than any other cause, and the incidence of AIDS mortality across cohorts and states is correlated to some extent with childhood Medicaid exposure: it was highest for those born in the 1950s, fell strongly for those born in the 1960s (who survived to benefit from ARVs), and was concentrated among nonwhite men in New York and New Jersey, two relatively high-AFDC states.[11] I use cause-elimination

[10] Furthermore, the structure of the 1980s expansions often makes it difficult to know why Medicaid affects shorter- and longer-run health and economic outcomes. While medical care use increased for pregnant mothers and children who gained new coverage (Currie and Gruber 1996a, b), it may have fallen among those who switched from private insurance to Medicaid (Currie and Gruber 2001). Crowd-out families also gained disposable income (Leininger, Levy, and Schanzenbach 2012) but faced incentives to draw down savings (Gruber and Yelowitz 1999). New Medicaid recipients were also adjunctively eligible for food benefits (Bitler and Currie 2004) and, in some cases, gained Medicaid coverage as a consequence of expansions in cash welfare eligibility. Both of these programs have been shown to have longer-run effects (Aizer et al. 2014, Hoynes, Schanzenbach, and Almond 2012).

[11] Empirically, the inclusion of New York and New Jersey is the primary reason why AIDS affects the estimates. Dropping these two states yields overall mortality results similar to those reported below and also yields null results on AIDS-related deaths.

life table methods (Manton and Stallard 1984) to construct cumulative mortality rates that reflect the force of non-AIDS mortality and not the effect of AIDS on the size of the population at-risk.[12] I construct other cause-specific mortality rates in the same way.

My second group of health outcomes is the set of disability variables in the 2000-2014 Census and American Community Survey (ACS). These are commonly used to measure health (Bound et al. 2003), and are extremely relevant to labor market outcomes. The types of disability are hearing or vision problems; difficulty with activities such as walking or carrying (ambulatory difficulty); difficulty going outside the home (mobility difficulty); difficulty getting around inside the home (self-care difficulty); difficulty learning, remembering, or concentrating (cognitive difficulty); and difficulty working at a job or business (work limitation). Because of changes in the question text, this analysis uses native-born respondents ages 25 to 64 born before 1976 in the 2000-2007 Censuses and ACS (Ruggles et al. 2010) collapsed to averages by race (r; white and nonwhite), birth state (s), and birth cohort (c).

I use the 2000-2014 Census and ACS to calculate rates of labor force participation, employment, cash transfer or public insurance receipt, average income by source, and the distribution of such income by source.

III. RESEARCH DESIGN: MEDICAID IMPLEMENTATION, CATEGORICAL ELIGIBILITY, AND CUMULATIVE ELIGIBILITY ACROSS STATES AND COHORTS

Cumulative Medicaid eligibility, m_{rsc}, may be spuriously correlated with adult outcomes if, for example, migration sorts healthier children to higher- or lower-eligibility states, or if changes in AFDC rates reflect demographic, economic, or policy conditions. To address these potential biases, I adapt the difference-in-differences strategy in Goodman-Bacon (forthcoming) and compare changes in outcomes across cohorts born in states with different child AFDC rates in the year of Medicaid implementation. This initial categorical eligibility rate, denoted $AFDC^*_{rs}$, provides a fixed ranking of states by which to compare adult outcomes and avoids comparisons between earlier and later Medicaid-adopting states, which differed on a range of characteristics.

[12] Let the 1980 population of cohort c from state s be $POP_{cs,1980}$, and denote annual AIDS-related deaths by $d^{AIDS}_{cs,y}$, and non-AIDS-related deaths by $d^{OTH}_{cs,y}$. It is straightforward to calculate cause-specific mortality rates *in 1980* as $mr^{AIDS}_{cs,1980} = \frac{d^{AIDS}_{cs,1980}}{POP_{cs,1980}}$, and $mr^{OTH}_{cs,1980} = \frac{d^{OTH}_{cs,1980}}{POP_{cs,1980}}$. Subsequent mortality rates can be calculated similarly using annual deaths in the numerators and the surviving cohort population, $POP_{cs,1980} - \sum_{j=1980}^{y}\left(d^{AIDS}_{cs,j} + d^{OTH}_{cs,j}\right)$, in the denominator. If period mortality rates would have been the same in the absence of other causes (ie. independent risks), then an estimate of the cause-elimination mortality rate from cause k is $1 - \prod_{j=1980}^{1999}\left(1 - mr^{k}_{cs,j}\right)$.

A. Evidence on the Validity of the Initial-AFDC Research Design

For comparisons based on initial AFDC rates to generate consistent estimates of Medicaid's effect, $AFDC_{rs}^{*}$ must predict cumulative eligibility (relevance) and be uncorrelated with other determinants of cross-cohort changes in adult outcomes (excludability). Figure 2 provides crude evidence on the strength of $AFDC_{rs}^{*}$ for predicting cumulative eligibility. The dashed lines plot average cumulative eligibility in states with above- and below-median values of $AFDC_{rs}^{*}$. The average difference across the state groups for fully treated cohorts is 0.47 years for white children (s.e. = 0.03) and 1.12 years for nonwhite children (s.e. = 0.13).

As to the second assumption, initial AFDC rates are plausibly excludable instruments because cross-state variation in welfare participation arose largely because of historical institutional factors unrelated to the circumstances facing cohorts first treated by Medicaid (Alston and Ferrie 1985, Bell 1965, Moehling 2007). To construct a high-powered test of the validity of the research design, I follow Kling, Liebman, and Katz (2007) and examine two indices of potential confounders. One combines child health measures using annual vital statistics data from 1950-1965, and the other combines childhood socioeconomic (SES) measures using data from the 1950-1970 Censuses. The components are coded to represent "good" outcomes, standardized using the mean and standard deviation from 1950, and averaged together with equal weight. I estimate the "shape" of any pre-Medicaid trends non-parametrically by regressing each index on year dummies interacted with $AFDC_{rs}^{*}$, and then regress the index on a linear trend interacted with $AFDC_{rs}^{*}$ to increase the power of the test to reject the null of no differential trends (as long as such trends are roughly linear). I also pool all years and test for cross-sectional balance in the level of each index. This avoids a series of potentially noisy balance tests that may produce a small number of significant correlations due to multiple testing.

Figure 3 plots the results and shows little evidence that changing circumstances faced by infants and children in the 1950s and 1960s are correlated with initial AFDC rates. Neither trends nor levels in the health or SES indices differ significantly for whites. The white health index in states that were 5 percentage points apart in $AFDC_{rs}^{*}$ would only have diverged by 0.17 standard deviations over 15 years (0.0022*5*15 = 0.165). The differential pre-trends and level differences in the nonwhite health index are smaller by an order of magnitude. $AFDC_{rs}^{*}$ has a small correlation with levels and trends in nonwhite SES. Still, the magnitudes are small: the nonwhite SES index in states that were 8 percentage points apart in $AFDC_{rs}^{*}$ (the standard deviation of nonwhite AFDC rates) would have diverged by just 0.25 standard deviations over 15 years (0.0021*8*15 = 0.25).

Appendix table A3.3 presents the results from three additional index-based balance tests. The first uses newly collected data on the incidence and eradication of polio (from the March of Dimes Archives) to show that one of the signature public health achievements of the 20th century, the dissemination of the Salk polio vaccine between 1954 and 1957, was uncorrelated with initial AFDC rates. The second test uses the 1963 Survey of Health Services Utilization and Expenditures to show that parents' self-reported willingness to seek care and views on the value of medical care are also uncorrelated with initial AFDC rates. Finally, I use data on the quality of dwellings and appliance ownership first available in the 1960 Census to show that home quality and durable goods consumption are also orthogonal to initial AFDC rates.

B. Event-Study Specification

A more direct test of the research design comes from reduced-form event-study models that trace out the relationship between adult outcomes and $AFDC^{*}_{rs}$ for cohorts born in different years relative to Medicaid ("event-cohorts"). The estimating equation for outcome Y is:

$$Y_{rsc} = \boldsymbol{X}'_{rsc}\boldsymbol{\beta} + AFDC^{*}_{rs}\left[\sum_{j=-(a+1)}^{-20} \pi_j 1\{c - t^{*}_{s} = j\} + \sum_{j=-18}^{b+1} \phi_j 1\{c - t^{*}_{s} = j\}\right] + \varepsilon_{rsc} \quad (2)$$

My preferred specification of $\boldsymbol{X}'_{rsc}$ includes fixed effects for state, cohort, and age; region-by-cohort fixed effects, to account for convergence in outcomes across U.S. regions unrelated to Medicaid (Chay, Guryan, and Mazumder 2009, Stephens and Yang 2013); and Medicaid-year-by-cohort fixed effects, to eliminate comparisons between earlier and later Medicaid-adopting states, which were on different trajectories both in terms of socioeconomic and health outcomes before Medicaid.[13] I also include the general fertility and infant mortality rates, per-capita income in each cohort's birth year, and the average number of hospital beds per-capita in each cohort's first 12 years. Identification in equation (2) comes from comparisons of respondents born in the same region in the same event-time across values of $AFDC^{*}_{rs}$. I cluster standard errors by birth state (48 clusters), and for key coefficients, I present *p*-values from a wild-cluster percentile-*t* bootstrap.

[13] Between 1950 and 1970, for example, white child poverty fell by about 21 percent in states that implemented Medicaid before 1969, but by 33 percent in states that implemented in 1969 or 1970 (s.e. of the difference is 2.3).

The coefficients of interest, π_j and ϕ_j, trace out changes in the relationship between $AFDC_{rs}^{*}$ and y_{sct} across event-cohorts relative to the omitted group, $j = -19$.[14] The π_j are falsification tests. Cohorts born more than 18 years before the introduction of Medicaid had no childhood coverage, so changes in their outcomes should not be related to initial Medicaid eligibility.[15] The ϕ_j are intention-to-treat (ITT) effects that measure the relationship between an additional percentage point of initial eligibility and changes in outcomes for cohorts first exposed $-j$ years after birth, i.e., at age $max\{0, t_s^{*} - c\}$. Because exposed cohorts are treated from age $max\{0, t_s^{*} - c\}$ to 18, each ϕ_j is analogous to a distinct experiment in which the Medicaid dose differs by $AFDC_{rs}^{*} \cdot (19 - max\{0, t_s^{*} - c\})$ *and* coverage begins at age $max\{0, t_s^{*} - c\}$.

These results can be used to test several features of both the treatment effects and the design. First, the ϕ_j will be zero if Medicaid has no effect when received at age $max\{0, t_s^{*} - c\}$ and older. The semiparametric event-study specification makes no assumptions about *when* coverage begins to matter, a salient issue in the literature on long-run effects. The ϕ_j do not separately identify heterogeneous effects by age at exposure versus amount of exposure, though, because cohorts who were young when Medicaid was passed also had more coverage. Conclusions about age versus amount of coverage require additional assumptions. Second, the pattern of the ϕ_j around $t_s^{*} = c$ (i.e., $j = 0$) provides an additional test of the design because all cohorts born after Medicaid have the same age (0) and amount of exposure (19 years). The ϕ_j should be similar for $j \geq 0$ if the effects are due to Medicaid because the "experiment" is the same for these cohorts.

C. Instrumental Variables Specification

To express the effects in terms of years of childhood eligibility, I estimate instrumental variables models that use the predicted "dose" described above as an instrument for actual cumulative eligibility m_{rsc}, defined in equation (1). The instrument, z_{rsc}, eliminates variation from annual changes in AFDC rates and migration and generates a predicted cumulative eligibility based only on initial AFDC rates in each cohort's birth state:

$$z_{rsc} = \sum_{y=c}^{c+18} 1\{y \geq t_s^{*}\} \cdot AFDC_{rs}^{*} = AFDC_{rs}^{*} \cdot (19 - max\{0, t_s^{*} - c\}) \qquad (3)$$

[14] I report coefficients for event-cohorts born between a years prior to and b years after Medicaid implementation. Cohorts born outside the event window [-a, b] are grouped into (unreported) terms for $-(a + 1)$ and $(b + 1)$.
[15] Members of these cohorts could still have qualified for Medicaid as public assistance recipients or through Medically Needy provisions, but survey data show a sharp drop in Medicaid use and eligibility after age 18.

IV estimates that use z_{rsc} as instruments for m_{rsc} estimate the average ITT effect of an additional year of cohort-level cumulative eligibility across ages of exposure. Splitting the eligibility variables by sub-periods of childhood allows a test of whether the average effect per year of eligibility differs by age at exposure.

IV. Intention-to-Treat Effects of Medicaid on Adult Health

Figure 4 plots first-stage event-study estimates of equation (2) that measure the relationship between $AFDC^*_{rs}$ and cross-cohort changes in migration-adjusted cumulative Medicaid eligibility, m_{rsc}. The coefficients for event-times -30 through -20 are small by construction: there is no relationship between $AFDC^*_{rs}$ and cumulative eligibility for cohorts with *no* childhood Medicaid exposure. The positive and increasing coefficients for event times -18 through 0 show that even after incorporating childhood migration, cohorts born in states with higher initial eligibility accumulate more childhood eligibility per year of exposure to any Medicaid program. The slope is twice as steep for whites as for nonwhites (0.007 versus 0.003), and column 1 of table 1 presents first-stage coefficients on z_{rsc} that confirm this difference. Initial eligibility strongly predicts white cumulative eligibility (0.72, s.e. = 0.16), but is only weakly related to nonwhite cumulative eligibility (0.20, s.e. = 0.18). The coefficients for event times 1 through 5 flatten out (whites) or erode (nonwhites), which underscores the earlier claim that the “dose” of childhood Medicaid exposure is the same for cohorts born after implementation. If Medicaid has long-run effects, then event-study estimates for other outcomes should have this pattern as well.

A. Cumulative Mortality, 1980-2000

The event-study estimates in figure 5 show that Medicaid reduces later-life mortality.[16] The white point estimates are small for cohorts with no childhood Medicaid exposure—a key test of the validity of the design. They are also small for cohorts that were only eligible in their teenage years, suggesting that later childhood eligibility has no effect on subsequent mortality. The coefficient on the interaction between $AFDC^*_{rs}$ and a linear event-time variable between -30 and -11 is small and insignificant (-0.02, s.e. = 0.02), implying that mortality would only differ by about 0.4 percent between cohorts born 20 years apart in states with a one percentage point difference in initial AFDC rates (1*-0.02*20). The estimates are negative and growing for white

[16] The dataset includes cohorts born as early as 1936—aged 64 in 2000 and born 30 years before the earliest Medicaid implementation date—and as late as 1975—aged 25 in 2000 and born five years after the latest Medicaid implementation year. This fixes the event-times at which I observe cohorts from all states: [-30,5].

cohorts with increasing exposure under age 10 (the linear trend break is -0.15, s.e. = 0.07), and they flatten out for those with full childhood exposure (0.23, s.e. = 0.11). Both features match the shape of the first stage and support the AFDC-based research design. The nonwhite trend-break estimates are similar. Results from the same specification used for whites show that these breaks are relative to an upward trend in mortality for the oldest cohorts (0.07, s.e. = 0.03). To address this, the nonwhite results plotted in figure 5 come from a two-step procedure in which I estimate the linear pre-trend on data through event-time -9 (the best-fitting trend break point), remove it from the full dataset, and estimate equation (2) on the adjusted data.[17]

To highlight the magnitude of these results, table 2 presents IV estimates of the effect of childhood eligibility separately for ages 0-10 and 11-18.[18] Column 1 shows the effects for all-cause mortality rates, and column 2 shows results for non-AIDS mortality that correspond to figure 5. The effects of early childhood eligibility are qualitatively similar for both outcomes but are about two-thirds as large for non-AIDS mortality and, for white adults, significantly more precise. Each year of childhood Medicaid eligibility is associated with a reduction in adult mortality of between 15 and 20 log points.

What size effects among treated adults are required to rationalize these ITT effects? In terms of a proportional reduction per year of coverage, this will be larger than the ITT since this population segment must account for *all* the averted deaths but may be smaller to the extent that adults with childhood Medicaid eligibility have more AFDC exposure (by definition) and higher baseline mortality. Appendix 8 shows how to use post-treatment data on mortality differences by poverty status (available in the National Longitudinal Mortality Study) and on childhood AFDC receipt to infer the proportional treatment effect on the treated that is consistent with the ITT in table 2. A year of childhood coverage reduces cumulative non-AIDS-related mortality rates by 8 percent among treated white adults and 10 percent among treated nonwhite adults.

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[17] A common strategy to deal with trends in a difference-in-differences design is to include unit-specific linear time trends. The event-study figures clearly show time-varying treatment effects, however, in which case unit-specific trends cannot distinguish between treatment effects and pre-existing trends (Lee and Solon 2011). My two-step procedure has no effect on the estimated trend *breaks*. It only alters the orientation of the event-study coefficients and the resulting IV estimates. I apply a degrees-of-freedom correction to the second-step standard errors to account for the (one) regressor estimated in step one.

[18] First-stage estimates for age-specific eligibility measures are in table 1, and age-group-specific first-stage event-study results are in appendix figure 2.1. The nonwhite age-specific first-stage estimates are stronger than the overall first stage but still reflect the previously mentioned changes in nonwhite AFDC rates. For instance, predicted eligibility under age 10 is *negatively* associated with actual eligibility between ages 11 and 18. Being born in a *low*-AFDC state in the 1960s means that nonwhites have low early childhood eligibility but relatively higher later childhood eligibility, which induces the negative correlation.

These reductions are larger than the other estimates of childhood Medicaid coverage on adult mortality. Wherry and Meyer (2013) find an ITT effect per year of eligibility of about -0.375 internal-cause deaths per 10,000 for black teens 15-18, which translates to a 6 percent decline in annual teenage internal-cause mortality per year of eligibility among the treated. The comparable ATET in Brown, Kowalski, and Lurie (2014) is about a 1 percent reduction in mortality between ages 18 and 27. These differences make sense. Both recent studies focus on eligibility later in childhood, and the treated subgroups, particularly in Brown, Kowalski, and Lurie's (2014) setting, which includes eligibility for non-poor children, whose families have higher incomes than those of categorically eligible children and who may benefit less. Finally, the 1980s expansions led to higher levels of childhood eligibility than those under initial implementation. A typical Roy pattern of selection implies the highest marginal treatment effects at lower levels of participation (Heckman and Vytlacil 2005).

The rest of table 2 presents estimates by cause of death. Columns 3 through 7 show that Medicaid's effects are strongest among the most plausibly affected conditions—internal causes—but that Medicaid worked through a range of conditions within this group. The internal cause estimates (column 3) are noticeably larger than the estimates for either non-AIDS-related causes or external-causes (column 7). The leading internal causes of death among adults other than AIDS were cardiovascular disease and cancer. Cardiovascular disease plays a more important role for nonwhite mortality than white mortality, consistent with its higher incidence among nonwhite adults. Both groups have similar cancer effects—a rare finding in the literature on life-course health. Behavioral changes could underlie this link, as could changes in the infectious origin of certain cancers. For instance, the bacterium *h. pylori* is the primary cause of stomach cancer, and human papillomavirus causes cervical cancer. Medicaid coverage may have altered the incidence of such conditions.

Strikingly, column 6 shows large reductions in suicide (-39.7, s.e. = 8.4 for whites, -39.3, s.e. =15.9 for nonwhites), although suicides account for a small share of the full effect. This result is consistent both with reductions in the burden of chronic illness as discussed in Case and Deaton (2015) and with Medicaid's positive effects on contemporaneous and later-life mental health (Finkelstein et al. 2012, Miller and Wherry 2014), both of which may reduce suicides.

These results suggest that about 345,000 lives were saved between 1980 and 1999—54,000 among whites and 291,000 among nonwhites. Even assuming a relatively low value of statistical life of $845,000 (the lower end of the confidence interval of Ashenfelter and Greenstone's (2004)estimates [table 2, converted to 2012 dollars]), this suggests that Medicaid's longer-run mortality reductions are worth at least *$291 billion*.

B. Self-Reported Disability

Results using an independent measure of health, self-reported disability, affirm that childhood Medicaid eligibility improves adult health. Figure 6 plots event-study estimates for the most common self-reported disability in the Census: difficulties with activities like walking, climbing stairs, reaching, lifting, or carrying (ambulatory difficulty).[19] The point estimates (multiplied by 100 so that the *y*-axis measures fractions of a percentage point) match the first-stage and mortality results closely. They are small for cohorts with no childhood Medicaid exposure, as well as for whites in cohorts first exposed after age 10 (the linear trend from -23 to -11 is -0.007, s.e. = 0.008) and nonwhites in cohorts exposed after age 4 (pre-trend = -0.002, s.e. = 0.003).[20] There are clear negative trend breaks after these ages (white: -0.021, s.e. = 0.01; nonwhite: -0.014, s.e. = 0.01). Crucially, the trends reverse for cohorts with full childhood exposure (i.e., born after Medicaid implementation). A test that these coefficients are equal, a direct prediction of the design, yields *p*-values of 0.99 (whites) and 0.27 (nonwhites).

Table 3 presents IV estimates for ambulatory difficulty across specifications, using the trend breaks in figure 6 to break up eligibility ages at age 10 for whites and age 4 for nonwhites. The model in column 1 includes only event-time dummies and $AFDC^{*}_{rs}$, and the effects of early childhood eligibility are negative and imprecise. My preferred specification (column 2) shows that each full year of early-life cumulative eligibility reduces white ambulatory difficulty rates by 3.84 percentage points (s.e. = 1.15) and nonwhite rates by 2.92 percentage points (s.e. -1.81). Column 3 shows that the white result is not sensitive to population weighting (Solon, Haider, and

[19] Because of changes in the text of the disability questions, these results use data from 2000-2007 only. This determines the event-time window because 64 year olds in 2007 were born 23 years before the earliest Medicaid implementation year (1966), and 25 year olds in 2000 were born five years after the latest Medicaid implementation year in the sample (1970). The results are not sensitive to including all survey years or to collapsing across survey years to the state-by-cohort level.

[20] The trend break estimates listed in the figure come from fitting linear trends with three sections. The pre-trend goes through zero at time -19 (like the event-study estimates), the phase-in trend begins somewhere between time -19 and -1, and the post-trend begins at zero, when all cohorts have full childhood exposure. I present estimates that maximize the *F*-statistic on the trend terms, thus remaining agnostic about the age at which exposure begins to have longer-run effects. *F*-statistics are plotted in appendix figure 2.4. As in figure 5, the nonwhite results are from a two-step procedure that adjusts the data by removing a positive pre-trend in disability rates between event-cohorts -23 and -4. This does not affect the estimated trend breaks, just the orientation of the coefficients.

Wooldridge 2015). The magnitudes fall only slightly with the inclusion of state-specific linear cohort trends (column 4), although the pattern of effects suggests that trends are not an appropriate control (Lee and Solon 2011).

The last two columns address the concern that characteristics of respondent's *current* state confound comparisons based on state of birth. Column 5 expands the dataset by state of residence and includes cohort-by-state-of-residence fixed effects. This controls non-parametrically for factors that vary across cohorts and states of *residence* (strongly correlated with birth state) such as age-varying effects of state policies, trends in chronic pain and opioid abuse (Case and Deaton 2015), AIDS incidence, and adult migration. Identification comes from changes across cohorts in the relationship between disability and $AFDC^{*}_{rs}$ for respondents who *live* in the same state. Column 6 further expands the dataset by survey year and controls for interactions between cohort and state-by-year unemployment rates to test whether self-reported disability is a response to labor demand conditions (Charles and Decicca 2008). Neither specification alters the conclusion that early Medicaid exposure reduces disability.

The effects of early Medicaid eligibility for white adults extend to all disability measures (panel A of table 4), pointing to broad improvements in functional capacity. Perhaps as a consequence of lower incidence of the relatively severe limitations, appendix figure 3.4 also shows a suggestive negative effect of early Medicaid coverage on the probability of living in group quarters (-0.38, s.e. = 0.22). The effects across types of disabilities are consistent with general improvements in health due to Medicaid but also underscore the difficulty in uncovering Medicaid's specific physiological channels.

Nonwhite results only appear for ambulatory difficulty, despite the fact that Medicaid's contemporaneous effects on child health were strongest and most precise for nonwhite children. The differences in short- and long-run effects may relate to aspects of Medicaid utilization other than crude eligibility rates. Even among those eligible for Medicaid in the 1960s, white children were 17 percentage points more likely to use medical care in a year than nonwhite children (65 versus 48 percent; Loewenstein 1971 table 2.1), and they saw private providers twice as often as nonwhite children (80 versus to 43 percent for most recent site of care; Loewenstein 1971 tables 2.45, 2.46, and 5.15).[21] In the short run, it may have been easy for simple medical care to save

[21] This matches direct reports about provider availability/access. When asked "Do you think that people who are eligible to get free medical care through their local welfare departments must go to certain places or can they go

nonwhite children's lives. Differences in *types* of care received by nonwhite kids may limit their long-run health benefits. Alternatively, because nonwhite adults experience higher disability and mortality rates than white adults, even if Medicaid's effects are similar by race, competing risks may work against observing this effect in the population (Freedman and Spillman 2016).

How big are these effects relative to counterfactual disability rates among treated children? Unlike mortality, disability rates for adults with child welfare receipt are observable in the PSID (University of Michigan Survey Research Center 2016). Forty-one percent of white children whose families received AFDC income in 1968 reported a work limitation as adults in 2001, compared to 15 percent on average. This implies that the actual rate of ambulatory difficulty among the treated subset is (5.7*41/15) 15.6 percent.[22] This number *includes* Medicaid's treatment effect, however. Analyses of PSID data (Smith and Yeung 1998) and administrative data on welfare spells (Berger and Black 1998) suggest that the average white child with any childhood AFDC spent about two full years on the program by age 10 (likely spread out over multiple calendar years). Therefore, the results in table 3 suggest that their adult disability rates are lower by (2*3.84) 7.7 percentage points. Adding this effect implies a counterfactual rate of ambulatory difficulty among the treated of 23 percent (5.7*41/15 + 3.84*2) and a proportional reduction in ambulatory difficulty per year of Medicaid eligibility among treated children of 16.5 percent. Similar calculations for nonwhite adults imply a reduction in adult disability of 13 percent per year of childhood coverage.

These results suggest large improvements in quality of life in response to Medicaid. Activity limitations are fundamental to many concepts of well-being itself (Sen 1993) and are closely related to self-reported happiness and satisfaction.[23] Furthermore, the disability and mortality results reinforce each other, since deaths from potentially disabling conditions fall and since reductions in disability may feed back to a reduction in suicide (Giannini et al. 2010).

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anywhere?" 61 percent of white categorically eligible household heads reported that they could go "anywhere," compared to only 46 percent of nonwhite household heads. White categorically eligible families were also twice as likely as nonwhite families to have switched providers in the previous two years (Loewenstein 1971). (There was no racial difference in recent provider switching among poor families in states that had not implemented Medicaid by 1968.)

[22] This calculation uses the ambulatory difficulty rate among those born from 1955 to 1975: 5.7 percent. The implied rate among the treated (15.6 percent) is actually lower than the rate among poor adults in the Census, 17.7 percent.

[23] The 2001 National Health Interview Survey (MPC and SHADAC 2012) shows that 21 percent of poor non-elderly adults with an activity limitation report being happy "a little" or "none" of the time. The figure for poor adults with no limitations is 8.6 percent and for non-poor adults with limitations is 12.7 percent.

C. Selective Survival

The preceding evidence suggests that Medicaid's effect on disability could be biased by changes in the composition of survivors. If Medicaid saved the lives of those who ultimately became disabled, then the estimates in tables 3 and 4 will understate Medicaid's effect on disability. The observed disability rate (dr_{rsc}) is the average of the rates among those who were saved by Medicaid (dr_{rsc}^1) and those who would always have survived (dr_{rsc}^0):

$$dr_{rsc} = (1 - p_{rsc}) \cdot dr_{rsc}^0 + p_{rsc} \cdot dr_{rsc}^1 \qquad (4)$$

p_{rsct} is the share of each cohort that survived to appear in the Census/ACS data because of Medicaid. I use the contemporaneous infant and child mortality estimates in Goodman-Bacon (forthcoming) and the cumulative adult mortality estimates from table 2 to construct true and counterfactual probabilities of surviving to the year 2000 and calculate p_{rsc} as their difference. Then, since dr_{rsc}^1 lies between 0 and 1, I bound the treatment effect on dr_{rsc}^0 using different assumptions about dr_{rsc}^1, similar to Bharadwaj, Løken, and Neilson (2013, table 4).

Row 1 of table 5 shows that selective survival plays almost no role in the treatment effects for white cohorts. Estimates differ by less than 5 percent across almost the full range of assumptions about dr_{rsc}^1. Rows 2-4 show that selective survival could explain much of the difference by race in the average effects under age 10. For low values of dr_{rsc}^1, the nonwhite effects are about half as large as the white effects (although not statistically distinguishable), and this difference falls to three quarters for the highest values of dr_{rsc}^1 (50 or 80 percent). Rows 3 and 4, however, show selective survival does not explain why the nonwhite effects are concentrated under age 4 instead of age 10. The survival adjustments magnify the ages 0-4 effect (it exceeds the white estimates for values of dr_{rsc}^1 of 30 percent or more) but never lead to economically or statistically significant effects for ages 5-10.

V. INTENTION-TO-TREAT EFFECTS OF MEDICAID ON ADULT LABOR MARKET OUTCOMES

The results in section IV imply that childhood Medicaid eligibility induces substantial improvements in adults' physical health. This section examines how these health improvements affect labor supply and income as well as public costs and revenues.

A. Labor Supply and Transfer Program Participation

Both event-study and IV estimates show that the positive health effects of childhood Medicaid eligibility translate into increased extensive margin labor supply and reduced transfer program participation for whites. The series with closed triangles in figure 7 plots event-study estimates of Medicaid's effect on annual employment. In line with all the other results, the trends for cohorts exposed to Medicaid after age 10 are flat (-0.002, s.e. = 0.011), and there is a clear trend break for the same cohorts that experienced health improvements (0.048, s.e. = 0.015), which is largely eliminated for groups with full childhood exposure (-0.035, s.e. = 0.016). IV estimates (table 6) show that each year of childhood Medicaid eligibility reduces the probability of being out the labor force by 6.59 percentage points (s.e. = 1.49) and increases employment (current: 5.82, s.e.=1.18; annual: 6.33, s.e. = 1.37), most of which is full-time/full-year (4.72, s.e. = 0.75). The 2001 PSID shows relatively small differences in employment between white adults with and without childhood AFDC receipt, which suggests that the implied 13-point increase in employment among treated children represents an 18 percent increase over their counterfactual employment rate of 72 percent (based on the employment rate among whites born from 1955-1975, 85 percent).[24]

Medicaid's effects on disability benefit receipt (Social Security Disability Insurance [SSDI] or Supplemental Security Income [SSI]; the series with open squares) are almost the mirror image of the labor supply effects and track the disability results closely. The pre-trend is small and insignificant (-0.012, s.e. = 0.012), there is a negative trend break for cohorts exposed at age 10 or younger (-0.022, s.e. = 0.015) and a positive one for cohorts with full exposure (0.036, s.e. = 0.007). The corresponding IV estimate in table 7 shows a reduction in disability transfer participation of -5.88 percentage points (s.e. = 1.54). Other welfare receipt (mostly Temporary Assistance for Needy Families, TANF) actually rises slightly (0.83, s.e. = 0.14). Disability benefits are higher than TANF benefits, meaning that, abstracting from non-negligible application costs, people who qualify for both prefer SSI/SSDI. Health improvements that disqualify households for disability benefits may simply lead some to take up TANF.[25]

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[24] Effects for nonwhite labor supply have similar signs, but, consistent with their smaller disability results, the magnitudes are much smaller and none are statistically significant. The rest of this section presents and discusses results for white adults.

[25] The appendices provide additional evidence on the validity of the design using employment and public assistance data from 1970, 1980, and 1990. These Censuses contain labor supply and program participation measures for much older cohorts during prime working years. I use these data to conduct two related falsification tests. Figure 4.8 uses the 1980 and 1990 Censuses to extend figure 7's pre-period to 45 years. There is no relationship between $AFDC^*_{rs}$ and employment or public assistance trends even for cohorts born in the 1920s. Figure 2.4 shifts event-time back for cohorts observed in the 1970 and 1980 Censuses and estimates "false" event-studies across the same *ages* used in the main analysis but in much earlier survey years. There is no evidence that age/employment or age/public-assistance patterns were correlated with $AFDC^*_{rs}$ for untreated cohorts.

The connection between cash and in-kind benefits means that Medicaid also has important intertemporal effects within the public insurance system. Column 4 of table 7 shows that cohorts with higher Medicaid eligibility in early childhood use public insurance *less* often as adults (-4.19, s.e. = 0.98).[26] The reduction in public coverage is largely offset by private insurance because these adults work more, but since about three quarters of these jobs are full time, total insurance coverage falls slightly (and insignificantly: -1.16, s.e. = 0.70).

These results speak directly to empirical analyses of disability insurance and benefit programs. Recent research uses random assignment of disability applications across evaluators with different award rates to show that, *holding health constant*, disability benefits reduce labor supply (French and Song 2014, Maestas, Mullen, and Strand 2013). Another approach decomposes time-series changes in disability receipt into its components *holding nothing constant*, and concludes that health improvements have had little impact on the rolls (Autor and Duggan 2006, Duggan and Imberman 2009). Reform proposals therefore emphasize ways to improve medical reviews, tighten eligibility criteria, smooth out the benefit structure (Autor and Duggan 2006), or increase administrative capacity (Liebman 2015). The results in tables 6 and 7, on the other hand, show that *holding program incentives constant* (through the cross-state comparisons), improvements in health greatly reduce disability benefit receipt and increase labor supply.[27] Multiplying the effect per year of eligibility (-5.88) by the population in affected cohorts (56 million whites with early childhood eligibility) and the average eligibility under age 10 (0.37 years) suggests that there are 1.2 million fewer SSI/SSDI recipients because of Medicaid implementation—about 15 percent of the average number of white, non-elderly recipients between 2000 and 2014.[28]

[26] This effect comes both from Medicaid, for which almost all SSI recipients are categorically eligible, and Medicare, which SSDI recipients can receive after a two-year waiting period. ACS data show that among non-elderly SSI recipients, 94% have Medicaid and 33% have Medicare, while among SSDI recipients, 33% have Medicaid and 44% have Medicare.

[27] That the effect of early Medicaid coverage on employment (6.33) is larger than its effect on disability assistance (-5.88) supports the claim that improved adult health is the main causal channel because even rejected disability applicants are quite unhealthy and work at low levels (Bound 1989). Underlying improvements in activity limitations would, therefore, tend to increase labor supply among *both* recipients and non-recipients of SSI/SSDI.

[28] These effects appear to get slightly stronger at older ages. Appendix figure 4.9 plots separate IV estimates for annual employment and disability benefit receipt for each of the 15 survey years. The design is unchanged, but the results describe effects for cohorts with early childhood coverage between their mid-30s and late-40s. For both outcomes, the estimate effects are larger when the relevant cohorts are older.

B. Sources of Income

Increases in labor supply and reductions in transfer program receipt offset each other in terms of income. Figure 8 plots a series of IV coefficients for eligibility between ages 0 and 10 for dependent variables defined as the probability of reporting earnings, transfer income, or total income greater than or equal to x. When $x = 0$, for example, the earnings coefficient measures the probability of any earnings—i.e., annual employment—and the transfer coefficient measures the probability of any transfer income—i.e., public assistance participation. As x moves up, the results trace out Medicaid's effect on the distribution of income by source.

The distributional analysis provides two important pieces of information about Medicaid's effect. First, the earnings effect is concentrated in the lower part of the distribution. Because income mobility for these cohorts is low (Chetty et al. 2014, Lee and Solon 2009), this lends further support to the claim that the effects are due to Medicaid's treatment of poor children. Second, the figure shows that increased earnings offset reduced transfer income. The positive earnings coefficients are larger than the negative transfer income coefficients, but the estimates for total income are insignificant. The effects for earnings between \$20,000 (the 99th percentile of transfer income in 2014) and \$55,000 (close to the mean for employed whites in 2014) remain positive and marginally significant, suggesting some effect on middle-class jobs.

Table 8 quantifies Medicaid's effect on average income by source.[29] Columns 1 and 2 show that average earned income increases, but the full-sample estimate is quite noisy. Trimming earnings above \$100,000 (as in Chetty et al. 2011) changes the point estimate by less than 3 percent but cuts the standard error by a factor of almost four, leading to the expected significant increase in earnings: \$1,810 (s.e. = 617). Transfer income falls by \$590 (s.e. = 162), and total income is higher (\$609), but not by a significant amount (s.e. = 654). Medicaid does not significantly reduce adult poverty rates (column 5) because it largely affects the *composition* of income rather than the amount.

C. Human Capital

Long-run research based on the 1980s expansions finds increases in educational attainment (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014), but table 9 shows no evidence that

[29] Because the effects in figure 9 are essentially differences in (one minus) the CDFs of non-negative random variables, their integral approximates Medicaid's effect on the mean of each income source. Summing each point times \$2,000 (the bin width) yields estimates very close to those in table 7: \$2,359 increase in earnings, \$1,092 increase in total income, and \$600 reduction in transfer income.

Medicaid implementation affected high school completion, college attendance, or college graduation probabilities. Multiplying the upper end of these confidence intervals by average early childhood eligibility (0.37) rules out cohort-level effects larger than 1.3 percent for high school graduation, 3.2 percent for college attendance, and 1.6 percent for college graduation.

While the null result on education differs from related research, human capital is not the most likely mechanism for the extensive margin labor supply effects documented above. Long-run health studies that link earnings to improved education tend to work through higher wages rather than extensive margin labor supply. Bhalotra and Venkataramani (2015), for example, find that the introduction of sulfa drugs increased income and education among white men but failed to affect employment rates (table 6, column 3). Similarly Southern black cohorts born around the time of the 1964 Civil Rights Acts experienced large improvements in test scores, educational attainment, and earnings, but not because of increases in employment rates (Chay, Guryan, and Mazumder 2009, 2014). Long-run evaluations of educational interventions come to similar conclusions. Chetty, Friedman, and Rockoff (2014), for example, find that extensive margin labor supply explains at *most* 23 percent of the long-run earnings effects of teacher quality.[30]

VI. DISCUSSION: THE RETURN TO MEDICAID SPENDING

The results above show large effects of early childhood Medicaid coverage on adult health, labor supply, and program participation, mainly for whites. Some of these benefits accrue to individuals. The longevity effects alone, for example, are very large even with a conservative estimate of the value of lives saved (at least $291 billion). Incorporating the value of improved physical and cognitive functioning would add significantly to this number. This is in stark contrast to recent welfare estimates from the Oregon Health Insurance Experiment showing that Medicaid coverage is worth *less* than its cost (Finkelstein, Hendren, and Luttmer 2015).

With respect to total personal income, these changes largely cancel out. Medicaid alters the composition but the not the amount of income, and recipients are not materially better off. This is similar to the program substitution identified by (Kline and Walters 2016). It also implies that

[30] This fits well with simple employment and wage comparisons by disability status and by education. The wage gap by high school graduation status ($6.50) is much larger than the wage gap by disability status ($2.20), but the employment gap is much larger by disability (45 points) than by education (25 points). Thus, the reasons why exposure to Medicaid had no effect on educational attainment remain interesting, but the effects on health, employment, transfer participation, and income are all much more consistent with improved health and reduced activity limitations than with higher human capital attainment.

disability programs successfully insure against poor health and by doing so limit the extent to which early health investments can ameliorate adult poverty.

In contrast to the null effects on personal income, the government gains tax revenue from the new earnings *and* saves on transfer payments. How big, then, are the aggregate changes in revenue relative to the cost of childhood coverage for these cohorts? I estimate Medicaid's effect on measures of total income and payroll taxes, as well as specific tax items such as the EITC using NBER's Taxsim 9.0 (Feenberg and Coutts 1993). I follow Agrawal and Hoyt (2016) in preparing the data and allocate family-level tax liability across people according to their share of income. Each year of early childhood eligibility increases average annual federal tax liability by $294 (s.e. = 153). The distribution of tax liability, however, spreads out because of the increase in extensive margin labor supply. Households with a counterfactual tax bill of zero either owe positive taxes or receive large EITC refunds. Recent work finds reductions in EITC payments (Brown, Kowalski, and Lurie 2014), which can occur if Medicaid moves earnings above the plateau range of about $18,000 or reduces fertility in a way that shifts down the EITC schedule. My results on extensive margin labor supply and the low level of new earnings, however, are consistent with increases in EITC. Multiplying the revenue results by the total years of early childhood eligibility (0.37 years*56 million whites with any eligibility) gives an annual increase in tax revenue of $6.1 billion.

The newly employed adults appear to have left transfer programs, however, and the government saves *all* of these foregone benefits. Each year of early childhood eligibility reduces transfer income by $590 (table 7, column 3), which implies an annual savings of $12.2 billion (56 million*0.37*-590)—twice as much as the new tax revenue. Other research on Medicaid has neglected its impact on public assistance, and the large public return from reducing transfers shows that this omission matters greatly for the future savings of child Medicaid coverage.

Reductions in public insurance participation also represent an important source of savings. Each year of early Medicaid eligibility reduces public insurance coverage by 4.19 percentage points (table 6, column 4), and the results for self-reported disability and disability transfer receipt suggest that most of those who leave public insurance would have qualified through disability provisions. Per-enrollee expenditures are very high for disabled recipients of Medicaid ($16,643; Kaiser Family Foundation 2012) and Medicare ($10,495; CMS 2013 table 3.6), but they are also strongly influenced by the right tail of spending. The *median* SSDI recipient on

Medicare, for example, spends between $2,000 and $5,000 (and is probably on the lower end of this range since more than 47% of recipients spend under $2,000), and the average spending within that category is $3,326. Using this as a benchmark for public insurance spending suggests that lower public medical costs save $2.9 billion per year (56 million*0.37*$3,326*-0.0419).[31]

Relative to the cost of covering cohorts born between 1956 and 1975, the implied 21.2 billion in annual savings as a result of this coverage is 16 percent of the original cost of coverage. To calculate these costs, I first use data on total expenditures from 1966-1975 (Goodman-Bacon forthcoming) since all of this spending applies to the cohorts studied here. I use CPS data (Flood et al. 2015) to calculate the share of child Medicaid recipients born before 1976 for each calendar year between 1976 and 1993 (when the 1975 cohort was 18), and multiply this by total child Medicaid spending in each year (CMS 2013 table 13.10).[32] This implies that it cost $132 billion (in 2012 dollars) to cover the relevant cohorts that contribute to the effects documented above.

Because the costs and long-run benefits are separated by several decades, discounting strongly affects the ultimate return calculations. The standard approach discounts annual benefits (2000-2014) and annual costs (1966-1993) to 2014 dollars, which yields an annual return of between 5.6 and 8.5 percent (average of 6.9 percent). If these effects approximate what similar policy changes would achieve today, then the government can expect to earn this return over a similar time frame at interest rates of 3 percent. This discounting assumption suggests that we have saved 104 percent of the original *discounted* cost just in the 15 sample years.

This exercise is less suited to examining the savings that the government has actually enjoyed because real interest rates that determined the cost of borrowing when these expenditures were made were often much higher than 3 percent. Nominal 10-year treasury bond rates, for example, were over 10 percent for the first half of the 1980s, when about one third of the nominal expenditures on these cohorts occurred. Following the method used by OMB to conduct cost-effectiveness analysis, I also discount the (nominal) costs and benefits using observed (nominal) 10-year treasury bond rates. This yields similar benefits but much higher costs, suggesting that

[31] These public savings accrue mainly to the federal government, meaning that Medicaid's long-run effects represent a substantial intergovernmental transfer. States paid roughly half the cost of Medicaid in the 1960s and 1970s, but the federal government recoups most of the savings through SSDI and Medicare.
[32] I interpolate the share from 1 in 1975 to the observed 1980 value, the first year the CPS asks about Medicaid.

the annual return is between 1.6 and 2.6 percent (average of 1.9 percent), and the government has saved 28 percent of the *true* cost of covering the original Medicaid cohorts.[33]

VII. CONCLUSION

This paper uses the original introduction of Medicaid combined with historical variation in welfare-based Medicaid eligibility across states to provide evidence on the effect of childhood insurance coverage on adult outcomes. Despite large contemporaneous effects and high participation, nonwhite children covered by Medicaid do not appear to experience significant changes in adult outcomes. White children, on the other hand, are healthier adults by a number of measures—cumulative mortality and self-reported disability—work more, and are less likely to receive public transfer benefits, particularly those tied to disability. These cohorts were not, however, differentially well off in childhood nor did they experience different underlying trends in early-life health or exposure to related public programs from the 1960s (Goodman-Bacon forthcoming). The results consistently show that coverage at younger ages, typically below age 10, matters the most. Since Medicaid coverage provided a broad range of medical services, the adult health effects, across causes of death and types of disability, are similarly widely distributed.

The health improvements themselves are certainly quite valuable to individuals, but the labor supply and program participation effects offset each other so that material well-being—poverty and total income—are unaffected. These changes, however, doubly benefit the government. I calculate that the government saves between 1.9 and 7 percent of the original cost of covering these cohorts every year, depending on the method of discounting used. Two-thirds of these savings come from reductions in cash and in-kind transfers, which have not previously been studied in this context. Early-life health programs can improve later-life health directly, have little effect on poverty because they crowd out public benefits, and yet generate significant public savings.

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[33] These calculations ignore distortions introduced by other methods of financing, such as increased taxes (Tax Foundation 1968).

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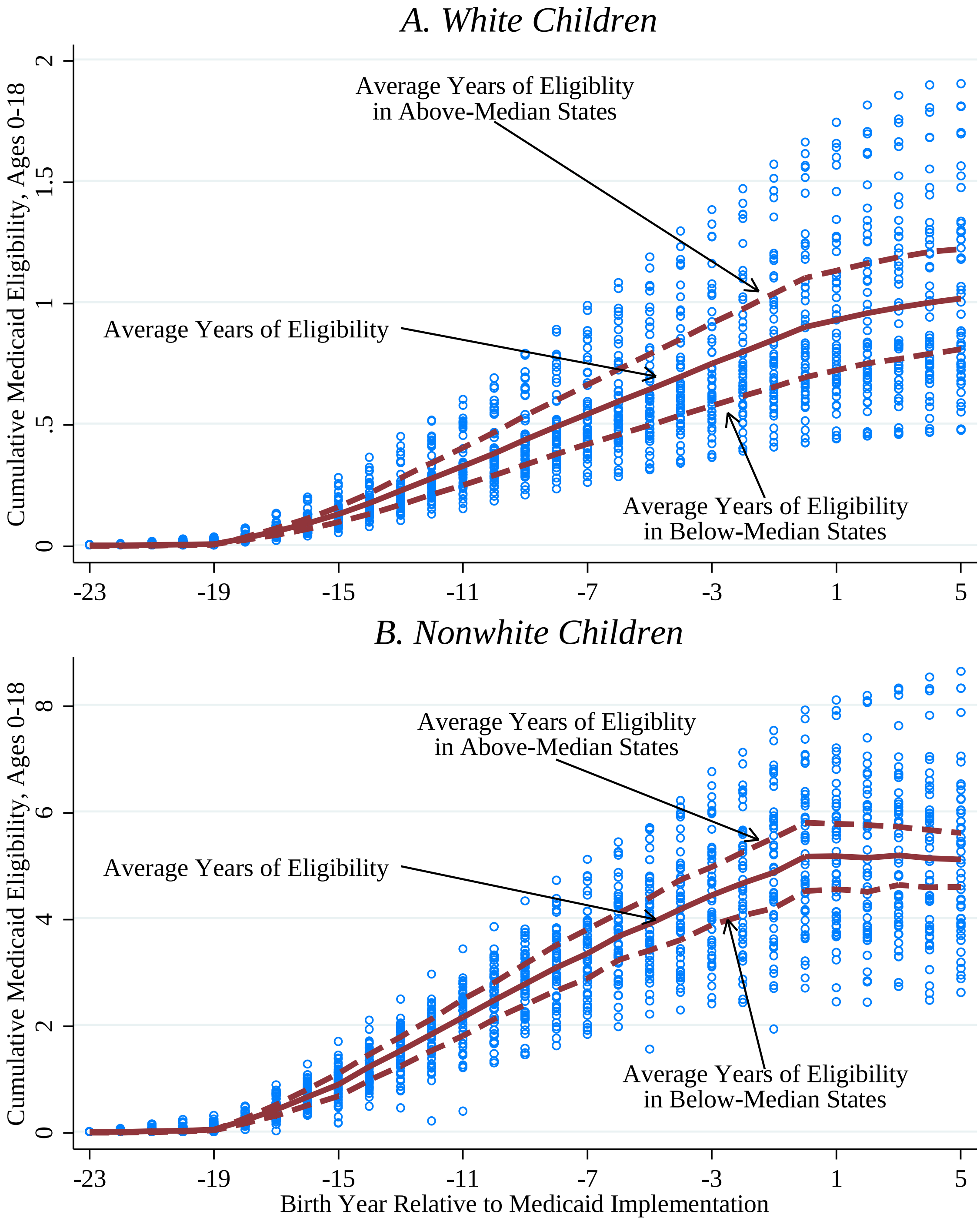
Figure 1. Family Income and the Probability that Children Saw a Doctor in the Previous Year, Before and After Medicaid

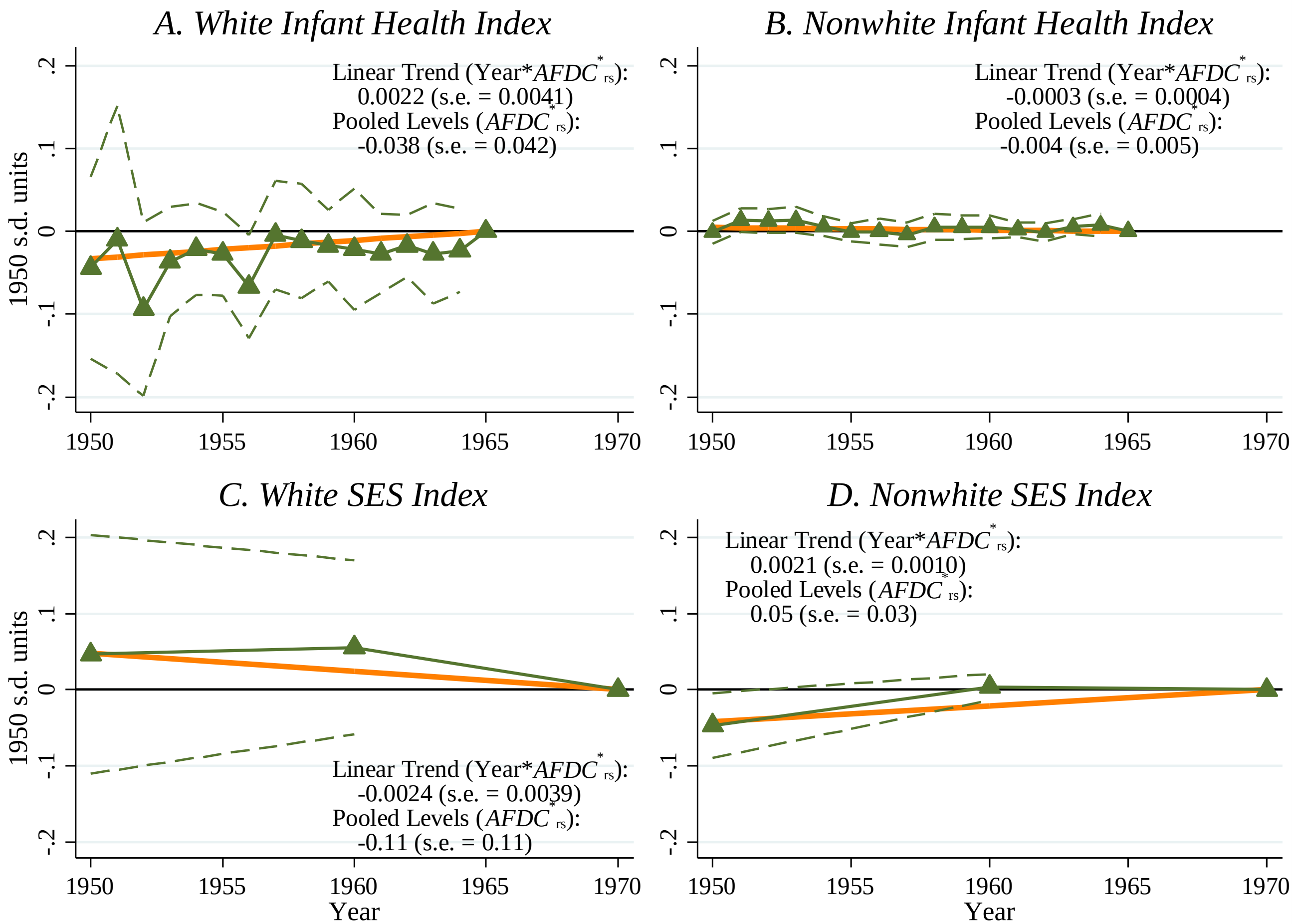
Notes: The figure plots the share of children who report having seen a doctor in the previous year in four survey data sources: the 1963 Survey of Health Services Utilization and Expenditure (CHAS 1988), the 1963-1965 National Health Examination Survey (ICPSR); and the 1963 and 1975 National Health Interview Surveys (NHIS). In all but the SHSUE, family income is reported as the median value of each bracket in which total family income is reported. In the SHSUE, it is the mean value within each decile. For scale, only bins less than or equal to $15,000 are plotted (income is measured in nominal dollars; the poverty line for a family of four is between $3,000 and $5,000). By this measure, income ceases to be a significant predictor of any annual doctor visit after Medicaid was implemented. The univariate regression slopes associated with these cell means are 0.027 (s.e. = 0.006) in the SHSUE, 0.027 (s.e. = 0.003) in the NHES, 0.029 (s.e. = 0.005) in the 1963 NHIS, and 0.0029 (s.e. = 0.002) in the 1975 NHIS. Given the clear nonlinearity in the pre-Medicaid years, the slopes on the observations of family income under $10,000 have the same pattern but are about twice as large (except for the 1975 slope: -0.004, s.e. = 0.004).

Figure 2. Cumulative Childhood Medicaid Eligibility by Race and Event Time



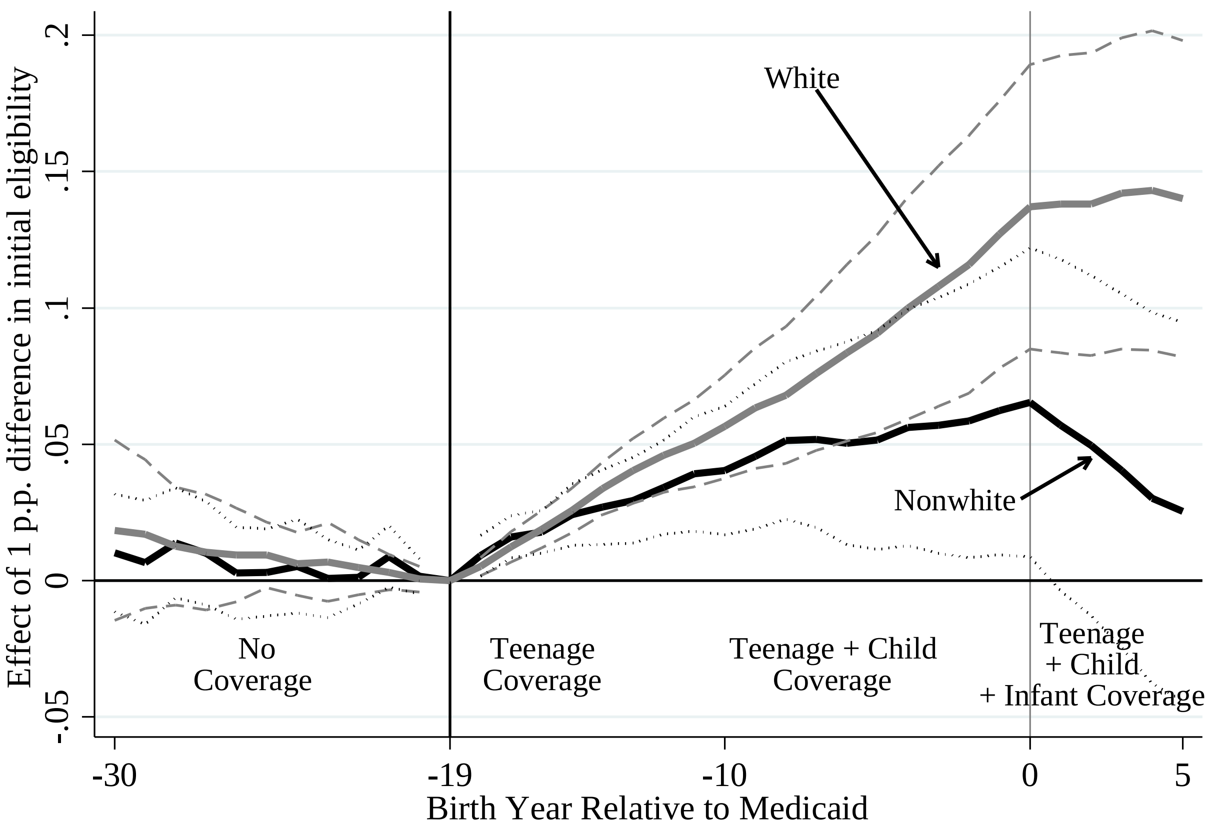
Notes: The figure plots cumulative childhood categorical Medicaid eligibility for birth cohorts from each state born in the 23 years before (1943-1947) and five years after Medicaid implementation (1971-1975). Note that the *y*-axes are different in the two panels. Cumulative eligibility is calculated according to equation (1) in the text. For each cohort, each year of childhood contributes a state-of-residence weighted mean of child AFDC rates interacted with a post-Medicaid dummy. These are summed over ages 0-18 to get an expected number of years of childhood eligibility. The solid line shows average eligibility across cohorts. The dashed lines show average eligibility in states with above- or below-median AFDC rates in the year of Medicaid implementation. For cohorts with full childhood coverage, eligibility differs across the two groups by 0.47 years for whites and by about 1.1 years for nonwhites.

Figure 3. Test of Differential Trends: Pre-Medicaid Changes in Indices of Infant Health and Socioeconomic Measures



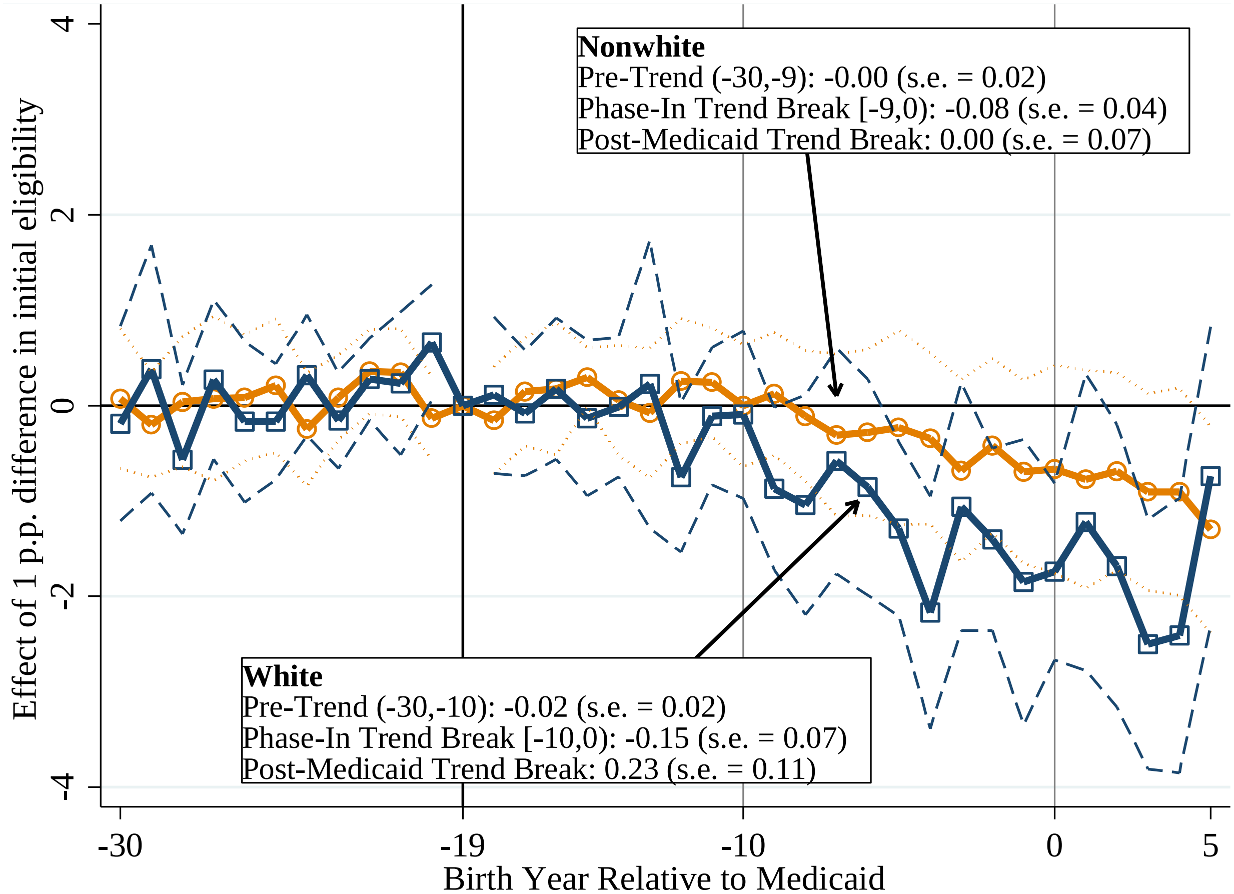
Notes: The infant health index is an equally weighted mean of the following variables standardized by their 1950 mean and standard deviation: low and very low birth weight rates, neonatal and postneonatal infant mortality rates, the sex ratio at birth, and the share of births in a hospital. The SES index is constructed similarly (for children under age 10) and includes the 25th, 50th, and 75th percentiles of children's household incomes; the child poverty rate; the share of children in households whose head has a high school degree or more, is in the labor force, and is employed; the share of children who live with no parents or both parents; household size; and the share of children ages 4-6 enrolled in school. The closed triangles are coefficients on the interaction between year dummies and $AFDC^*_{rs}$, and the straight lines are the estimated coefficient on an interaction between continuous year and $AFDC^*_{rs}$. The estimated slope and standard error are noted in the figure. The coefficient for "pooled levels" comes from a bivariate regression of the index on $AFDC^*_{rs}$. Regressions are weighted by births or the sum of Census weights, and standard errors (and the dashed 95-percent pointwise confidence intervals) are clustered by state.

Figure 4. First-Stage Relationship Between Cumulative and Initial Medicaid Eligibility Before and After Medicaid Implementation



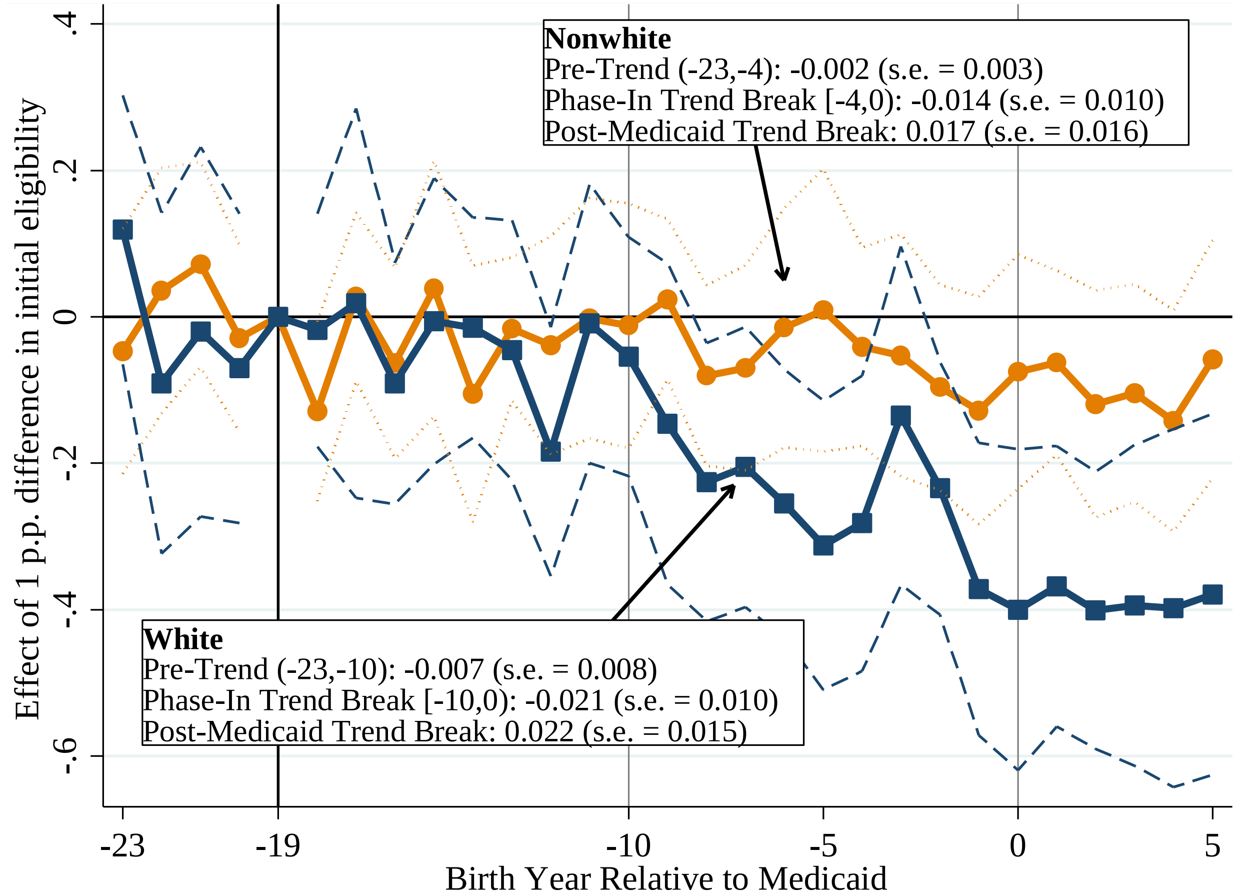
Notes: The dependent variable is each cohort's cumulative, migration-adjusted Medicaid eligibility for ages 0-18. The figure plots the estimated coefficients on interactions between $AFDC^{*}_{rs}$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The dataset includes one observation per state/year cohort because childhood eligibility is determined by age 18. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; state per-capita income and hospital beds averaged over childhood; and each cohort's general fertility rate and infant mortality rate. The dashed lines are 95-percent confidence intervals based on standard errors clustered by birth state. While the above/below median differences in eligibility in figure 2 are larger for nonwhites than whites, the effect per point of the AFDC rate is smaller both because of the model's controls and because the underlying AFDC differences across high- and low-AFDC states are much larger for nonwhite than for white AFDC rates.

Figure 5. Reduced-Form Event-Study Estimates of Medicaid's Effect on log 20-Year Non-AIDS Mortality Rates (coefficients × 100), 1980-1999



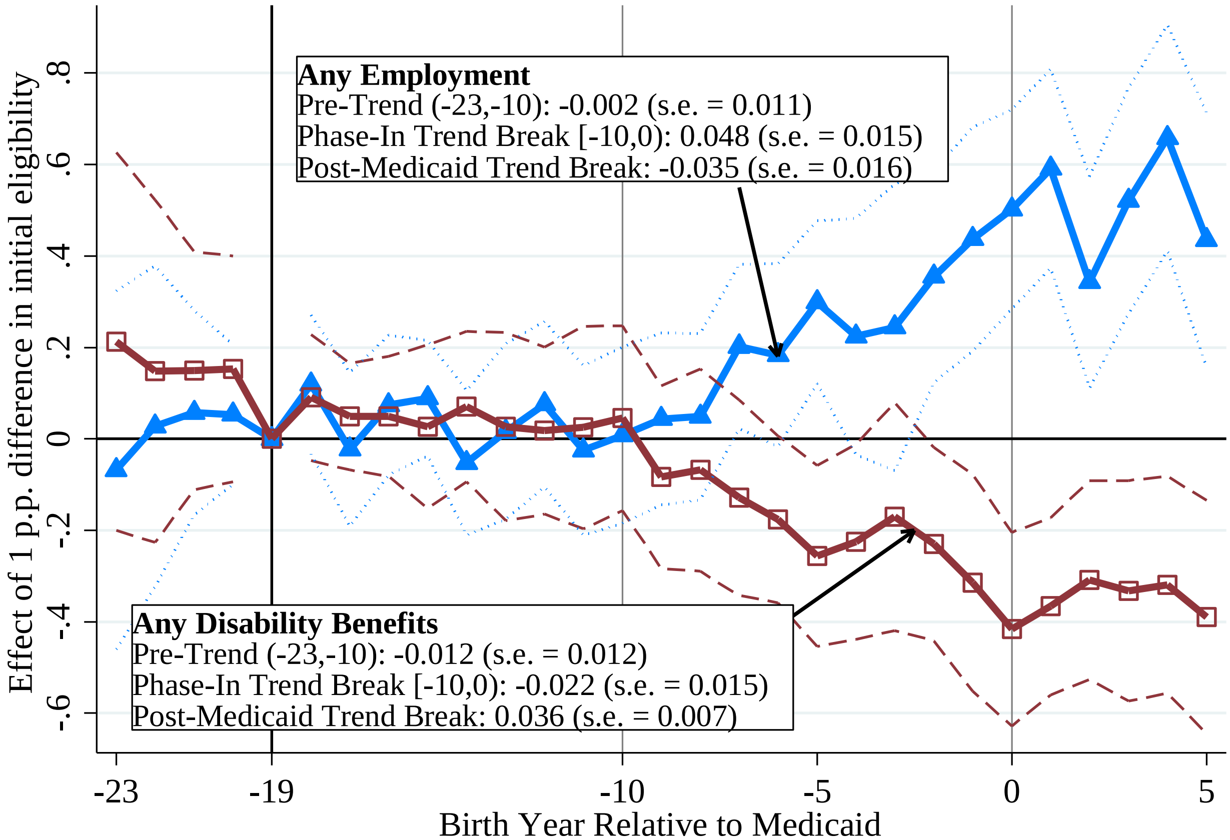
Notes: The figure plots the estimated coefficients on interactions between $AFDC^*_{rs}$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; state per-capita income and hospital beds averaged over childhood; and each cohort's general fertility rate. The nonwhite estimates also adjust for a linear trend interacted with $AFDC^*_{rs}$ for event-times prior to -10. Estimates are weighted by the 1980 population. The dashed lines are 95-percent confidence intervals based on standard errors clustered by birth state. The break at zero is fixed because all subsequent cohorts are exposed to Medicaid for their entire childhoods. Source: Ruggles et al. (2010), United States Department of Health and Human Services (2009).

Figure 6. Reduced-Form Event-Study Estimates of Medicaid's Effect on Adult Rates of Ambulatory Difficulty by Race (coefficients×100)



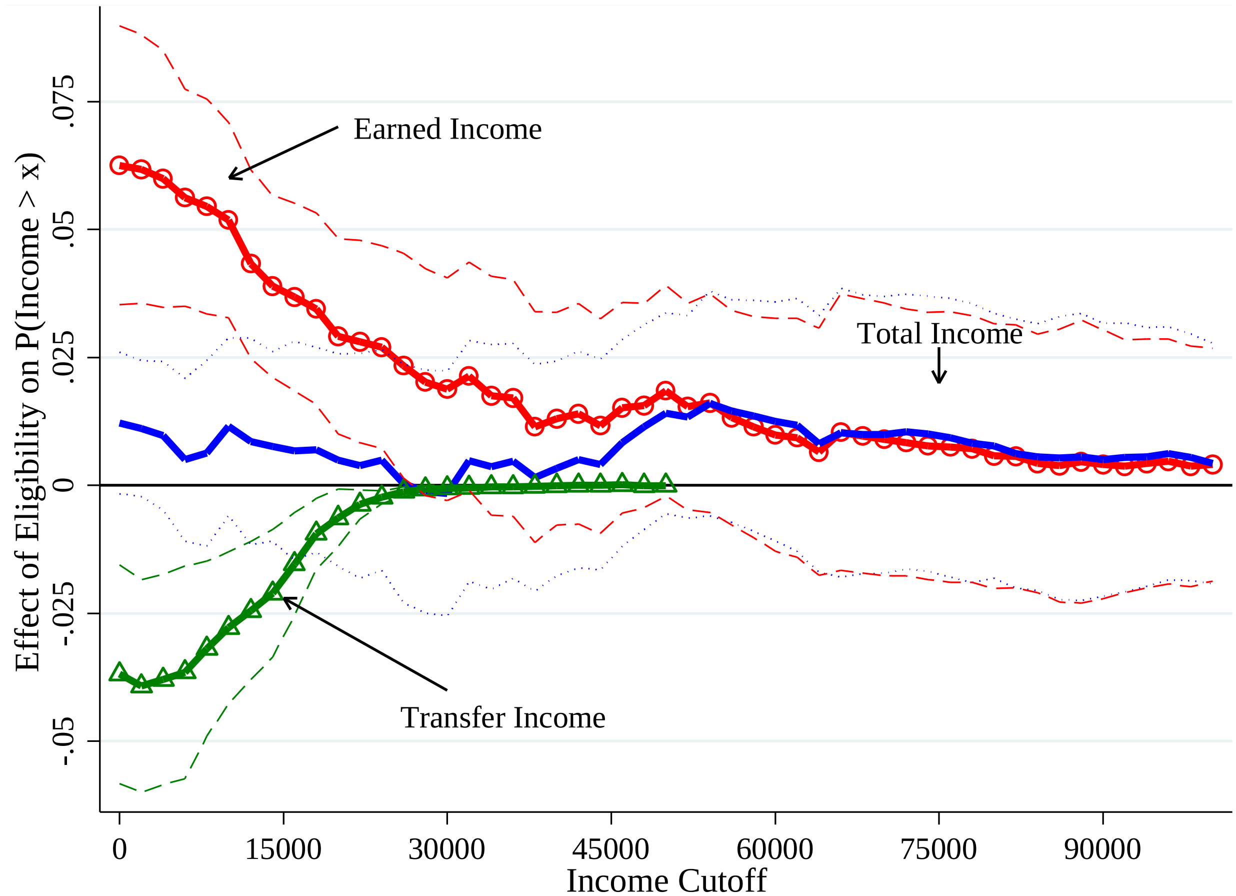
Notes: The dependent variable is the share of respondents in each state-of-birth-by-cohort cell who report having a "long-lasting condition that substantially limits one or more basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying" (ambulatory difficulty). The estimation sample includes Census/ACS years 2000-2007, when the question text was comparable (see appendix figure 1.3). The figure plots the estimated coefficients on interactions between $AFDC^{*}_{rs}$ and event-time dummies for each of 23 years before and five years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; state per-capita income and hospital beds averaged over childhood; and each cohort's general fertility rate and infant mortality rate. Estimates are weighted by the sum of the Census weights in each cell (unweighted estimates are similar and plotted in appendix figure 3.1). The dashed lines are based on standard errors clustered by birth state. The trend break coefficients come from a model that fits straight lines between event times [-23, -11], [-10,-1], and [0,5]. The break at zero is fixed because all subsequent cohorts are exposed to Medicaid for their entire childhoods. The break at -10 comes from maximizing the *F*-statistic on the three trend terms that use different break points from -22 through -2. A plot of these F-statistics is in appendix figure 2.4.

Figure 7. Reduced-Form Event-Study Estimates of Medicaid's Effect on Employment Rates and Disability Benefit Receipt, White Respondents (coefficients×100)



Notes: The dependent variable is the share of white respondents in each state-of-birth-by-cohort cell who report having any annual employment (closed triangles) or receiving income from a disability-related transfer program such as SSI or SSDI (open squares). The estimation sample includes Census/ACS years 2000-2014. Because these questions are comparable over time, appendix figure 4.8 presents similar results using the 1980 and 1990 Census, which allows for a 45-year pre-trend (not all covariates are available for these cohorts). The estimates are nearly identical, and neither employment nor disability benefit receipt exhibit trends correlated with initial AFDC for cohorts born as early as 1920. For details on the specification, see text and notes to figure 6.

Figure 8. Instrumental Variables Estimates of the Effect of Medicaid Eligibility Before age 10 on the Distribution of Income By Source, White Respondents



Notes: The figure plots instrumental variables estimates of the effect of cumulative Medicaid eligibility under age 10 on the probability of earnings, transfer income, or total income greater than the amount on the *x*-axis (measured in $2,000 bins in 2012 dollars). The sample includes Census/ACS years from 2000 to 2014. $50,000 is the maximum of the transfer income variable.

Table 1. First-Stage Relationship between Predicted Eligibility and Migration-Adjusted Cumulative Medicaid Eligibility

	(1)	(2)	(3)
	Cumulative Eligibility, Ages 0 -18	*Cumulative Eligibility, Ages 0 -10*	*Cumulative Eligibility, Ages 11 -18*
		A. White Adults	
Predicted Eligibility at:			
Ages 0-18	0.72		
	[0.16]		
Ages 0-10		0.77	0.08
		[0.22]	[0.07]
Ages 11-18		-0.04	0.57
		[0.12]	[0.14]
F-statistic	21.7		
Angrist/Pischke *F*-statistic		33.0	13.1
		B. Nonwhite Adults	
Ages 0-18	0.20		
	[0.18]		
Ages 0-10		0.40	-0.42
		[0.20]	[0.08]
Ages 11-18		-0.02	0.57
		[0.08]	[0.14]
F-statistic	1.2		
Angrist/Pischke *F*-statistic		19.7	13.1

Notes: Column 1 presents first-stage estimates for the effect of predicted childhood Medicaid eligibility, z_{rsc}, on actual, migration-adjusted cumulative childhood Medicaid eligibility, m_{rsc}. Columns 2 and 3 present similar first-stage estimates that split eligibility into two sub-periods: ages 0-10 and ages 11-18. F-statistics that measure the strength of the age-specific instruments for each eligibility variable are presented for these regressions (Angrist and Pischke 2009).

Table 2. Instrumental Variables Estimates of Medicaid's Effect on log 20-Year Mortality Rates by Race and Cause (coefficients×100)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All-Cause	Non-AIDS-Related Causes					
Cause of Death:		Total	Internal (incl. Suicide)	Cardio-vascular	Cancer	Suicide	External (Homicide + Accidents)
Childhood Medicaid Eligibility				*A. White Adults, 1980-1999*			
Ages 0-10	-23.9	-15.5	-31.7	-15.0	-19.0	-39.7	-1.9
	[9.8]	[5.4]	[6.9]	[9.6]	[9.0]	[8.4]	[6.7]
	(0.058)	*(0.006)*	*(0.002)*	*(0.168)*	*(0.120)*	*(0.002)*	*(0.776)*
Ages 11-18	7.2	-11.0	-8.6	31.1	1.5	0.9	-10.4
	[14.3]	[7.2]	[7.2]	[10.8]	[7.8]	[11.2]	[14.3]
H_0: 0-10 = 11-18 (*p*-val)	0.19	0.70	0.08	0.01	0.13	0.03	0.65
Mean Dependent Variable (deaths per 100,000)	3,690	3,090	2,280	800	937	293	826
Childhood Medicaid Eligibility				*B. Nonwhite Adults, 1980-1999*			
Ages 0-10	-30.7	-19.6	-25.2	-27.3	-17.3	-39.3	-18.2
	[13.3]	[9.4]	[7.6]	[11.1]	[8.3]	[15.9]	[10.1]
	(0.020)	*(0.040)*	*(0.002)*	*(0.022)*	*(0.052)*	*(0.002)*	*(0.054)*
Ages 11-18	10.0	4.8	7.8	6.7	2.2	-5.0	4.0
	[10.7]	[7.0]	[6.9]	[8.3]	[7.2]	[9.7]	[7.2]
H_0: 0-10 = 11-18 (*p*-val)	0.05	0.06	0.01	0.04	0.11	0.06	0.11
Mean Dependent Variable (deaths per 100,000)	7,980	5,600	3,910	1,880	1,360	203	1,830

Notes: The table presents instrumental variables estimates of Medicaid's effect on log cumulative mortality rates (1980-1999) by cause. Standard errors are clustered by state of birth, and for effects between ages 0 and 10, *p*-values from 500 draws of a percentile-*t* wild cluster bootstrap are in parentheses. Mortality rates by cause do not add to the total because they are calculated using cause-elimination life table methods to account for the confounding influence of competing risks from the other causes.

Table 3. Instrumental Variables Estimates of Medicaid's Effect on Adult Rates of Ambulatory Difficulty by Race and Across Specifications (coefficients×100)

	(1)	(2)	(3)	(4)	(5)	(6)
Childhood Medicaid Eligibility			*A. White Adults, 2000-2007*			
Ages 0-10	-1.56	-3.84	-3.15	-2.34	-2.75	-3.80
	[1.69]	[1.15]	[1.49]	[1.16]	[0.76]	[1.15]
	(0.480)	*(0.004)*	*(0.134)*	*(0.226)*	*(0.010)*	*(0.010)*
Ages 11-18	1.63	-1.13	-0.99	-0.42	0.45	-0.96
	[2.62]	[1.43]	[1.46]	[1.26]	[1.07]	[1.41]
H_0: 0-10 = 11-18 (*p*-val)	0.02	0.21	0.40	0.23	0.04	0.18
Observations		1,962			94,434	455,057
Childhood Medicaid Eligibility			*B. Nonwhite Adults, 2000-2007*			
Ages 0-4	-1.33	-2.92	0.11	-2.10	-2.08	-3.51
	[0.93]	[1.81]	[1.74]	[0.96]	[1.31]	[1.97]
	(0.168)	*(0.048)*	*(0.966)*	*(0.058)*	*(0.150)*	*(0.070)*
Ages 5-18	0.04	-0.16	-0.37	0.32	-0.24	-0.30
	[0.44]	[0.71]	[1.23]	[0.56]	[0.86]	[0.7]
H_0: 0-4 = 5-18 (*p*-val)	0.11	0.05	0.82	0.01	0.03	0.04
Observations		1,962			60,362	176,543
Covariates	Initial eligibility + Time-to-Medicaid FE	(1) + State FE+ Year FE + Medicaid-Year-by-Cohort Fe + Region-by-Cohort FE + Xst		(2) + State-specific cohort trends	(2) + Cohort-by-state-of-residence FE	(2) + Cohort-by-Year-by-Unemployment-rate interactions
Weighted?	Y	Y	N	Y	Y	Y
Level of Observation		State-of-birth/cohort			State-of-residence/state-of-birth/cohort	State-of-residence/state-of-birth/cohort/year

Notes: The table presents instrumental variables estimates of Medicaid's effect on ambulatory difficulty across specifications. Standard errors are clustered by state of birth. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the early childhood effects are in parentheses. The age ranges are determined by the best-fitting trend breaks from figure 6. Nonwhite results come from a two-step procedure in which a linear pre-trend from event-time -23 to -4 is removed and IV estimates are based on these adjusted data.

Table 4. Instrumental Variables Estimates of Medicaid's Effect on Adult Disability Measures by Race (coefficients×100)

	(1) Ambulatory Difficulty	(2) Hearing/Vision Difficulty	(3) Mobility Difficulty	(4) Self-Care Difficulty	(5) Cognitive Difficulty	(6) Work Limitation
Childhood Medicaid Eligibility			*A. White Adults, 2000-2007*			
Ages 0-10	-3.84	-1.17	-1.36	-1.24	-1.70	-2.99
	[1.15]	[0.29]	[0.36]	[0.29]	[0.39]	[0.74]
	(0.012)	*(0.004)*	*(0.006)*	*(0.002)*	*(0.008)*	*(0.004)*
Ages 11-18	-1.13	0.28	-0.69	0.35	0.31	-2.14
	[1.43]	[0.72]	[0.57]	[0.49]	[0.63]	[1.16]
H_0: 0-10 = 11-18 (*p*-val)	0.21	0.07	0.35	0.02	0.03	0.53
Mean Dependent Variable	8.61	3.15	3.75	2.27	4.41	8.50
Childhood Medicaid Eligibility			*B. Nonwhite Adults, 2000-2007*			
Ages 0-4	-2.92	0.28	-0.40	0.03	0.01	0.602
	[1.81]	[0.8]	[0.69]	[0.53]	[0.84]	[1.08]
	(0.054)	*(0.748)*	*(0.606)*	*(0.974)*	*(0.994)*	*(0.600)*
Ages 5-18	-0.16	-0.20	0.27	0.01	-0.33	0.35
	[0.71]	[0.47]	[0.36]	[0.31]	[0.46]	[0.59]
H_0: 0-4 = 5-18 (*p*-val)	0.05	0.34	0.19	0.98	0.59	0.75
Mean Dependent Variable	12.70	4.03	6.53	3.93	6.87	13.00
	Does this person have any of the following long-lasting conditions:		Because of a physical, mental, or emotional condition lasting ≥ 6 months, does this person have any difficulty:			
Question Text	...substantially limits ≥1 basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying?	Blindness, deafness, or a severe vision or hearing impairment?	Going outside the home alone to shop or visit a doctor's office?	Dressing, bathing, or getting around inside the home?	Learning, remembering, or concentrating?	Working at a job or business?

Notes: The table presents instrumental variables estimates of Medicaid's effect on all disability measures available in the Census. The specification is the same as in figure 6 and column 2 of table 3. There are 1,962 observations. The age ranges are determined by the best-fitting trend breaks from figure 6. Nonwhite results use a procedure in which a linear pre-trend from event-time -23 to -4 is removed and IV estimates are based on these adjusted data. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the early childhood effects are in parentheses.

Table 5. Instrumental Variables Estimates of Medicaid's Effect on Disability, Adjusting for Selective Survival

	(1)	(2)	(3)	(4)	(5)
Disability Rate Among Those Induced to Survive	*0%*	*15%*	*30%*	*50%*	*80%*
Childhood Medicaid Eligibility					
White, Ages 0-10	-3.83	-3.86	-3.90	-3.94	-4.01
	[1.15]	[1.15]	[1.15]	[1.15]	[1.16]
Nonwhite, Ages 0-10	-1.77	-2.08	-2.40	-2.82	-3.45
	[1.3]	[1.29]	[1.29]	[1.29]	[1.3]
Nonwhite, Ages 0-4	-3.09	-3.61	-4.13	-4.82	-5.85
	[2.04]	[2.02]	[2.02]	[2.02]	[2.05]
Nonwhite, Ages 5-10	-0.26	-0.35	-0.44	-0.55	-0.73
	[0.87]	[0.86]	[0.86]	[0.85]	[0.85]
H_0: Row 1 = Row 2 (*p*-val)	0.23	0.30	0.38	0.51	0.74
H_0: Row 1 = Row 3 (*p*-val)	0.75	0.91	0.92	0.70	0.42

Notes: The table presents IV estimates from two regressions that pool white and nonwhite observations and use outcome variables adjusted for differential survival (see figure 5) under different assumptions about the disability rate of induced survivors. All covariates, instruments, and exogenous variables are fully interacted with white and nonwhite dummies so that the coefficients are comparable to those in table 3. In model 1 (rows 1 and 2), both white and nonwhite eligibility are measured from ages 0-10 and 11-18. In model 2, white eligibility is the same (row 1) and nonwhite eligibility is measured from ages 0-4, 5-10 (rows 3 and 4), and 11-18. The *p*-values come from *t*-tests of the null hypothesis that the early childhood effects are equal across race. Moving across rows shows the role of selective survival in the white/nonwhite differences in the results. Survival has no effect on the white results. Under the assumption that Medicaid saved the lives of nonwhite adults who would have been very disabled, the nonwhite treatment effects are almost twice as large, but as the event-study results in figure 6 show, they are still present only for exposure under age 4.

Table 6. Instrumental Variables Estimates of Medicaid's Effect on Extensive Margin Labor Supply for White Adults (coefficients×100)

	(1)	(2)	(3)	(4)
	Out of the Labor Force	Currently Employed	Any Employment Last Year	Full-Time/Full-Year Employment
Childhood Medicaid Eligibility				
Ages 0-10	-6.59	5.82	6.33	4.72
	[1.47]	[1.18]	[1.37]	[0.75]
	(0.002)	*(0.002)*	*(0.002)*	*(0.002)*
Ages 11-18	1.46	-2.10	-0.94	-2.44
	[2.00]	[1.95]	[1.97]	[1.93]
H_0: 0-10 = 11-18 (*p*-val)	<0.01	<0.01	<0.01	<0.01
Mean Dependent Variable	22.0	74.2	80.9	51.4

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.

Table 7. Instrumental Variables Estimates of Medicaid's Effect on Public Assistance Receipt for White Adults (coefficients×100)

	(1)	(2)	(3)	(4)	(5)
	Any Public Assistance	Disability Benefits	TANF or General Assistance	Public Insurance	Any Insurance
Childhood Medicaid Eligibility					
Ages 0-10	-5.09	-5.88	0.83	-4.19	-1.16
	[1.50]	[1.54]	[0.14]	[0.98]	[0.70]
	(0.020)	*(0.014)*	*(0.002)*	*(0.002)*	*(0.138)*
Ages 11-18	1.95	2.23	-0.19	0.77	4.04
	[3.48]	[3.40]	[0.29]	[1.62]	[1.33]
H_0: 0-10 = 11-18 (*p*-val)	0.09	0.05	0.00	0.00	0.00
Mean Dependent Variable	15.8	15.1	1.0	13.5	7.7

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.

Table 8. Instrumental Variables Estimates of Medicaid's Effect on Average Income by Source for White Adults (coefficients×100)

	(1)	(2)	(3)	(4)	(5)
	Earned Income	Earned Income (Trimmed: $100k)	Transfer Income	Total Income (Trimmed: $100k)	Poverty Rate
Childhood Medicaid Eligibility					
Ages 0-10	1,870	1,810	-590	609	1.16
	[2,134]	[617]	[162]	[654]	[1.01]
	(0.592)	*(0.008)*	*(0.002)*	*(0.366)*	*(0.612)*
Ages 11-18	-3,756	-2,348	-512	-3,975	2.23
	[3,144]	[964]	[416]	[801]	[0.70]
H_0: 0-10 = 11-18 (*p*-val)	0.26	0.00	0.84	0.00	0.34
Mean Dependent Variable	45,088	31,945	1,026	35,441	7.5

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.

Table 9. Instrumental Variables Estimates of Medicaid's Effect on Educational Attainment for White Adults (coefficients×100)

	(1) High School Grad	(2) Any College	(3) Bachelor's Degree
Childhood Medicaid Eligibility			
Ages 0-10	1.27	1.55	0.57
	[1.1]	[2.91]	[1.91]
	(0.496)	*(0.676)*	*(0.828)*
Ages 11-18	0.20	-1.47	-2.07
	[1.81]	[3.21]	[1.56]
H_0: 0-10 = 11-18 (*p*-val)	0.66	0.57	0.35
Mean Dependent Variable	91.7	62.6	31.4

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.